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LECTURES

ON THE

DISEASES OF THE BONES AND THE JOINTS.

BY

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LECTURE V.

Suppuration in bone, unlike that in soft parts, attended with increase of pain. Ulceration and absorption of cranial bones. Loss of bone from too great use of mercury, not syphilis. Treatment of cranial inflammation. Diseases of the facial bones consequent on bad condition of or injurious interference with the teeth. Necrosis of the jaw in scrofulous children. Spina ventosa. Ulceration of the atlas and axis. Mode in which death occurs. Treatment of such cases. Disease of the dorsal and lumbar vertebræ; importance of careful diagnosis. Lumbar and psoas abscesses. Occasional cure of caries of the spine. Treatment of psoas and other chronic vertebral abscesses. Injurious effect of mechanical interference.

THE *bones of the cranium* are often the seat of inflammatory action. The patient feels severe pain, first in one part, and then in another, of the scalp, and the affection seems, for a time, to shift from place to place. Soon, however, a puffy swelling of the scalp is observed, which is discoloured, hot, and very tender to the touch. Perhaps the centre of the swelling is soft and fluctuating, from the presence of fluid, and when this is the case the pain is generally very intense. In the softer tissues, when once inflammation terminates in suppuration and matter begins to form, there is some abatement of the pain; but in inflammation of bone the case is just the reverse, and the formation of pus is marked by very great aggravation of the suffering, owing, most probably, to the dense and unyielding nature of the tissues which bind down the fluid, and the great amount of pressure which is consequently exerted. The most usual situation for pus to collect, in inflammation of the cranium, is between the bone and the pericranium; and, as a frequent consequence, we find death of a portion of the bone thus separated from its natural investment. More rarely, however, the bone ulcerates, and you get what is called "worm-eaten caries" of the skull.

Sometimes, again, the bone is extensively destroyed by ulceration, and this absorption of osseous matter occasionally takes place to such an extent that nothing remains, in certain situations, but membranous structures, to protect the brain, which may be seen pulsating. This is a very troublesome and intractable complaint, and occurred more frequently formerly than in the present day. It used to be called "corona veneris," but was more attributable to the excessive administration of mercurial medicines than (as was supposed then) to the taint left by the venereal poison. I do not believe that a patient ever lost the bones of the head or face from syphilis alone. You will generally find, on inquiry into these horrid cases of caries of the skull, &c., that mercury has been pushed so far as to saturate the system, as it was said. In this state, some slight blow, perhaps, on the skull, is sufficient to commence the mischief which afterwards becomes so extensive.

During the active stage of these affections of the cranium the treatment must, of course, be antiphlogistic, consisting in the application of leeches and fomentations, incision of the pericranium, with or without division of the scalp, blisters, &c. Means are also taken to ameliorate the general state of health of the patient, which is, as I have said, frequently indifferent. When the inflammatory stage begins to subside the strength will require support to make up, as it were, for the discharge which takes place. The part itself should be left quiet, and after due attention has been paid that the openings in the skin are so situated as to allow of the ready escape of matter, instead of favouring its accumulation, the case should be merely watched until the dead bone begins to loosen. To ascertain when this is the case, examination is made

from time to time with a probe, and when the edges are felt loose from the living bone, means must be had recourse to for removing the exfoliation, even though the necrosed piece cannot be moved from side to side. In these cases it is generally the outer table of the cranium only which dies, and this is held on to the inner table by atmospheric pressure until such time as the edge is raised at one part and the air allowed to get under, then it separates easily. For this purpose, after a proper incision has been made, you try to lay hold of the edge of the dead bone, or insinuate an elevator under it, and turn it out by the leverage thus afforded you. Sometimes it is necessary to fix a screw into one of the holes in the bone and thus get a firm hold enough to raise it off. When this is removed granulations rise, and the patient speedily gets well. If the whole thickness of the bone is destroyed the space is filled up with thick and tough membrane, which performs the same office as the bone, namely, that of protecting the brain.

The *bones of the face* are subject to the same disease. The ossa nasi, the nasal processes of the frontal and superior maxillary bones, the vomer, &c., are sometimes entirely destroyed by this ulceration, or parts of them are thrown off in a necrosed state. This affection is generally preceded by inflammatory swelling and ulceration of the soft parts, and is attended with severe pain and offensive discharge. Such patients are unapproachable from the excessively fetid state of their breath, which is loaded, as it were, with putrid effluvia in passing over the diseased bones. Sometimes the spongy bones and septum are destroyed by degrees, and then the arch of the nose falls in and there is great deformity of that feature in consequence. All this may be produced by similar causes to those mentioned as bringing on disease of the bones of the cranium. Occasionally disease at the roots of the incisor teeth (which approach very near to the cavity of the nose) is at the bottom of all the mischief, though, at first, it may not be suspected. Again, the palate bones may either be thrown off in a necrosed state or be destroyed by ulceration. When this occurs it is attended with loss of voice, to a certain extent, and difficulty of deglutition. The patient has great trouble in swallowing food, and part of it (particularly when liquid) regurgitates into the back part of the nasal cavity, owing to the imperfect manner in which the velum pendulum performs its valve-like office. Indeed, in most of these instances where the palate bones are diseased, the soft parts themselves are more or less implicated in the ulceration, and an extensive breach of tissue is thus produced. Lastly, the bones of the face, particularly the superior maxilla, are subject to morbid growths, of which I shall not now speak, as we shall hereafter come to consider them under the head of tumours, and in relation to diseases of particular parts, as the antrum of Highmore, the mouth, &c.

Disease of the lower jaw is by no means uncommon in consequence of a bad state of the teeth. These are overlooked and allowed to get into a carious state; they break away, perhaps, and leave a ragged stump, which, after lying dormant for some time, causes, at last, irritation and inflammation of the sockets, and thus lays the foundation for more extensive mischief, such as abscess in the jaw, necrosis of the alveolar processes, &c. Inflammation, also, frequently follows the operations of the dentist, such as pivoting the incisor teeth. In this proceeding the crown of the tooth is removed and a false crown is fixed into the remaining part by means of a pivot, which is pushed into the cavity formerly occupied by the pulp of the tooth. Now this is sometimes followed by active inflammation of the pulp, extending to the socket, which runs to such an extent as to terminate only in the exfoliation of the alveolar process, and the loss, perhaps, of several other teeth in consequence.

Again, the body of the lower jaw may be the seat of necrosis; and this is not unfrequently met with in scrofulous weakly children, from slight causes, such as inflammation produced during the shedding the temporary teeth and progressive development of the permanent. This irritation, which is slight, and of little consequence in healthy children, goes further than necessary in such cachectic subjects, and, spreading from the gums to the bone, causes severe inflammation, and ultimately, death of the osseous tissue. Sometimes the evil advances to such an extent that the whole body of the bone and parts of its rami are destroyed, and, occasionally, nothing of it remains alive excepting the condyles and coronoid processes, which are rarely necrosed. In these extreme cases the mischief is irremediable, and the patient is obliged to manage without a lower jaw. Where, however, only a part of the bone is lost, or where the necrosis does not extend through its entire thickness, there is a secretion of new bony matter to fill up the cavity, and the second set of teeth spring up in their usual places, or nearly so. This, as you may suppose, does not take place without very great suffering to the little patient, and much care and trouble on the part of the surgeon. Abscesses generally form in different parts of the face and gums, which require to be opened from time to time, and the discharge of pus will be kept up, perhaps for a long time, either outside the cheek or into the mouth, or both. When the pieces of bone become loosened (which will be ascertained by examination with a probe) they should be removed through these openings. But the surgeon should be careful, under these circumstances, not to attempt the removal of

the sequestra before they are perfectly loose, as, by the irritation and inflammation consequent upon the operation, he is likely rather to increase than ameliorate the disease. Of course, during this treatment, as much must be done as possible towards getting the child into a better state of health.

Again, in adults, a considerable amount of disease may, in like manner, be produced by what appears slight causes, such as the presence of a stump, a crowded state of the teeth, &c. Inflammation of the gum, with swelling, throbbing pain, and tenderness of the soft parts comes on, attended, perhaps, with swelling of the face, pain in the head, &c. This may be followed by what is called *parulis*, or *gum-boil*, which is a collection of matter in the gum, and when the pus is evacuated there may be an end to all irritation. If, however, the *cause* be allowed to remain, there will, in all probability, be a repetition of the same symptoms again and again, until, at last, perhaps some of the deeper structures are involved in the disease.

Abscess in the cavity of the lower jaw (or *spina ventosa*, as it is absurdly enough called) is thus occasionally brought on. It consists merely of a collection of pus between the plates of the hard bone of which the jaw is formed. As it increases in quantity the cellular or cancellated structure is opened out or destroyed, the outer plates separate from one another, and become thinner and thinner as they get further apart. At last the lower jaw degenerates into little more than a thin osseous and membranous bag of pus. Of course there is great swelling and deformity of the face, and more or less disturbance in the functions of mastication and speech. The swelling feels hard, and, when pressed on, does not yield in any way. It may be mistaken for solid tumour of the bone, but, perhaps, on taking out a stump, a little matter oozes out, and the true nature of the case comes to be known. If the case be only slightly advanced the extraction of a stump or a tooth may be sufficient to allow the ready escape of matter, and the abscess (for it is really nothing more) gradually contracts and closes up with granulations, the swelling of the face subsides, and the patient recovers. But if the collection be large the cure is not easily brought about, and more active measures become necessary. The object, as in the case of other abscesses, is to obtain a sufficiently free and dependent opening for the ready draining off of the fluid, and the best means of arriving at this is to make an opening through the base of the jaw, and keep this open by a few threads of silk drawn through and tied on the outside. This may be done by taking out a tooth or stump and passing a needle, or a narrow, but strong bistoury, into the cavity, through the thin layer of bone and the soft parts beneath. A probe, armed with the requisite number of threads, is then passed through, along the knife, and the seton tied loosely, as usual. This practice I have found very successful, even in advanced cases, where it would appear almost impossible that the parts could ever regain anything like their natural size. It may sometimes be necessary to cut away a part of the bony parietes in order to evacuate the matter more freely.

Spina ventosa and other diseases of the jaw may be produced by abscess commencing in the fangs of the teeth. This may not be suspected, and is probably only discovered when the tooth is removed. A patient suffers violent pain in a tooth, or, perhaps, in a stump, which, when extracted, is found to have attached to its extremity a rounded smooth sac. It appears at first like a solid tumour, but, further examined, it is found to contain pus and to be lined by a delicate secreting membrane, like an ordinary abscess. Should such a tooth not be extracted, this cyst may burst, and the matter find an exit by the side of the tooth and between it and the gum.

There are other diseases of the jaw, such as epulis, osteo-sarcoma, &c., which will come to be considered hereafter under the head of diseases of particular parts, and in reference to the operations about the face and jaw which are necessary in some of these diseases.

For the present we pass on to consider

Diseases of the Spine.

The *cervical* vertebræ are sometimes the seat of disease, causing curvature of the neck, either laterally or forwards. This *may* depend on a curvature of the lower part of the vertebral column, the bend of the neck being only sufficient to keep the head still over the centre of gravity. It may, however, be caused by actual disease in the cervical vertrebæ themselves. This disease consists in ulceration or caries of the bodies of the vertebræ, and, perhaps, partial destruction of the inter-articular substance. The vertebræ most commonly thus affected are the atlas and axis, and the disease is more frequently seen on the left side than on the right. The patient is, in the first place, seized with stiffness and pain in the neck; there is also, perhaps, some swelling in the upper part of the neck; all which symptoms are probably looked on as of a trifling nature. Besides this, there is difficulty of swallowing, owing to the pressure of the swelling on the upper part of the pharynx. After a time the head begins to bend forwards and to one side (generally to the right side), and ultimately the chin rests on the sternum and the head is fixed. Patients labouring under this disease have a very characteristic appearance. The head is immoveable, and generally supported and steadied by the hand; the countenance is anxious and expressive of pain; and the eyes seem to have a greatly increased power

of motion to make up, as it were, for the deficiency in the ordinary amount of mobility in the head. As a natural consequence of the continuance of this disease, abscess forms in the neighbourhood of the pharynx, and generally opens into it. Through these openings portions of dead bone from time to time make their escape, together with more or less fetid discharge. If the patient be kept very quiet and the head steady, life may be preserved for a considerable length of time. Sooner or later, however, the disease almost uniformly death causes, and when that occurs it is generally very sudden. The manner in which it happens is easily understood. The patient, from some indiscretion in moving too suddenly, allows his head to drop, the softened and disorganised ligaments give way, or the odontoid process is broken off, one vertebra slips over the other, and, the spinal cord being suddenly compressed, death is instantaneous. Where the case lingers, paralysis of the lower, and even the upper limbs, may exist for some time before death.

The *treatment* of these cases consists in keeping the head *perfectly fixed* and applying antiphlogistic measures to the neck. The head may be fixed by means of a strap attached to a curved iron, which iron should be firmly fixed in a belt or stays round the body. At the commencement of the disease blood should be abstracted from the back of the neck by, cupping or leeches, after which counter-irritation by means of issues, &c., may be employed with advantage. The patient should also be kept lying down, and should be made aware of the danger of changing his position. Here is a specimen of a case in which disease of the articulating processes of the cervical vertebræ underwent a natural cure by ankylosis. This, however, is a very rare occurrence.

The *dorsal and lumbar vertebræ* are likewise subject to ulceration and consequent curvature; most frequently in children, though occasionally in persons of more advanced life. It *may* arise from injury, in the first place, and you generally find the parents insisting on referring the curve of the spine to some fall received at a distant period. By far the most common cause, however, is scrofulous deposit taking place in the cellular structure of one or more vertebræ. This is generally softened, the outer crust of the body of the vertebra gives way, and the consequence is a falling in of the column at this point and a sharp projection backwards of the corresponding spinous process. This "gibbosity" can never be mistaken by any one who takes the trouble of making an examination. It is important that it should be recognised, because the kind of treatment which this excurvation requires is very different from that which is proper in cases of gentle hoop-like curvature from softening only. In sharp curvature there is generally little or no counter-curve; the body bends forwards, the patient walks with difficulty, sometimes even supporting himself with his hands on his knees, or is quite disabled from standing long together. Still there is seldom any marked paralysis of the lower extremities or bladder, the spinal canal preserving its shape and size tolerably well. It is true that the legs and thighs are often in a great measure wasted and thin, but that is more from disuse than positive disease of or pressure on the nervous communications. Now, you will sometimes find very extensive disease of this kind without the formation of abscess. There is only a small quantity of pus immediately round the diseased bone, and bathing, as it were, the ulcerated parts. More frequently, however, if the disease continues long, suppuration is extensive, and large collections of matter form in front of the spinal column, which gravitate along the sides of the pelvis and make their way, ultimately, into the upper part of the thigh, or point more directly backwards at one side of the spinal column and open in the lumbar region.

In the majority of instances of caries of the bodies of the vertebræ, collection of pus takes place in front of the spine. This sometimes forms a large sac at the side of the vertebræ, makes its way through the soft parts between the ribs, and ultimately points in the lumbar region. This is what is called "lumbar abscess." Sometimes, however, matter extends down the side of the spine, guided, as it were, by the psoas muscle to the upper and inner part of the thigh, where it appears in the form of a fluctuating elastic swelling, which gives an impetus or coughing, in many cases at least. In such cases you have an example of what surgeons call abscess *in* a part in contradistinction to abscess *of* a part. When the matter takes this course the disease is called "psoas abscess." It points generally under the pubal portion of the fascia lata, in the upper third of the thigh; sometimes it extends, however, as far as the middle of the thigh. The disease is, of course, much complicated, and the danger increased, by the formation of these large abscesses, and even though nature may make an effort to remedy the mischief, the patient generally sinks under the immense and wasting discharge which takes place when the matter comes to the surface.

With regard to the terminations of caries of the spine a cure is not impossible. It occasionally happens, under favourable circumstances, that bony deposit takes place and the deficiency caused by the ulceration is completely made up. Several vertebræ are thus glued together, as it were, and a solid column is formed, containing even as many as ten or twelve bones in one. Far more frequently, however, the ulceration

continues slowly and steadily, destroying the bodies, but leaving the canal nearly healthy. Sometimes parts become necrosed, and you will find a sequestrum in the middle of a carious vertebra. In such circumstances there can be little chance of recovery, as the dead bone will keep up the discharge like any other foreign body. I should say, however, that necrosis of the vertebræ is a very rare occurrence.

Treatment.—The same principles which I laid down in the treatment of disease of the cervical vertebræ are applicable in the present class of cases. In the first place, the patient should be kept perfectly quiet, so that the growing together of the parts may be encouraged. Perhaps the prone position, about which so much has been said lately, is the most favourable, as it takes the pressure off the diseased parts, and prevents the carious bodies from falling in upon one another. It also assists the return of blood from the numerous veins contained in the bodies of the vertebræ, and in the spinal canal. In the early stages of the disease, if you are fortunate enough to be consulted at that period, counter-irritation may be employed with advantage. Caustic issues should be made over the loins, and a moderate amount of discharge kept up, not, of course, to an extent to weaken the patient, but so as to establish a kind of drain in the neighbourhood of the disease, and prevent suppuration where it is most to be feared. When abscess forms internally these issues should, however, be allowed to heal up, as they can then only increase the amount of suppuration, and perhaps the local irritation also.

But what is to be done with the abscess? This is a question about which there is much difference of opinion. Many surgeons are strongly averse to meddling with chronic abscesses,—they would rather leave them to take their own course, point where they will, and open by ulceration. That this is an erroneous practice in most instances I am firmly convinced. It must be evident that the larger the cavity is the more profuse and tedious will be the suppuration, and the longer will the cyst of the abscess be in contracting and becoming obliterated. The best course, therefore, which can be pursued to make the mischief as small as possible, is to provide a free exit for the pus when once it is fully formed. It generally happens in the lumbar or psoas abscess that the collection has attained a considerable size before the surgeon is consulted. The patient, perhaps, has been suffering for a long time under disease of the spine, and he presently notices a little swelling and stiffness in the groin, which increases slowly without exciting much attention. It is not till the swelling has become so large that progression is interfered with, that advice is sought for. It then seems a formidable thing to evacuate so large a quantity of matter, and many naturally feel disinclined to do it. Some would follow Abernethy's plan of opening the abscess with a lancet, pushing it in obliquely, so as to make a valvular aperture, and allowing it to close up as soon as a certain portion of the fluid has escaped. It is thought that this plan allows the cyst to contract gradually, whilst it prevents the entrance of air into the cavity, and, also, that it is less likely to produce depression of the vital powers than when the whole is evacuated.

However, I have so frequently seen severe irritative fever, and the worst of consequences follow this method of proceeding, that I would recommend you at once to make a free and dependent opening, and allow as much pus to escape as will flow out without pressure. The opening, if made sufficiently freely, need not be plugged with lint, as is usually the practice. A poultice may be applied at first, and then water-dressing will be sufficient. The discharge thus gradually subsides, and the cyst contracts upon its contents, as in the case of other abscesses. Whether it close completely, or whether a certain amount of discharge continue to lower the patient's strength, must, of course, depend upon a variety of circumstances connected with the progress, favourable or otherwise, of the original disease of the spine. Occasionally, as I have said, the affection of the spine is remedied, the discharge ceases, the patient gradually recovers his strength, and the lower limbs even regain their former size and strength. I need hardly say that in this disease any attempt to lessen the angular deformity by stretching the spine is most absurd, and can only cause laceration and destruction of any new matter which may have been thrown out on the carious parts. Irreparable mischief is thus done by quacks, who promise to the unfortunate patient what they call a "perfect cure."

Frequency of Ruptured Uterus.—As to symptoms of rupture of the uterus, they, like its causes, may be various; there are some, however, which are rarely absent and can never be mistaken. It generally happens that everything has been going on well for some time, when, all of a sudden, the woman cries out that something has given way within her, which sensation is quickly succeeded by great prostration of strength, vomiting, cessation of the pains, and recession of the presentation. These symptoms may *all* be present, or the woman may not have the slightest premonitory symptoms whatever. We find, likewise, that these symptoms may be, and frequently are, present in females whose labours terminate favourably.—*Dr. Mitchell, in Dublin Med. Jour.*

HOSPITAL FOR THE CURE OF DEFORMITIES.

COURSE OF LECTURES

ON THE

DEFORMITIES OF THE HUMAN FRAME.

BY

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Delivered at the Orthopædic Institution, Bloomsbury-square, in 1843.

Lecture V.

Replacement of articular surfaces in angular knee-ankylosis. The apparatus which is adapted for cases of false ankylosis of the knee without displacement. Knee-extending instruments. Restoration of movements in the joints. The advantage of tenotomy in knee-ankylosis. Complete immobility of ankle. Deformity of the ankle from injury, scrofula, and rheumatism, resembling talipes equinus, varus, and valgus.

In *false ankylosis of the knee, without displacement*, the simplest apparatus, (see last lecture, page 143), may suffice; but when displacement and rotation of the head of the tibia, with eversion of the leg, exist, a better adaptation of mechanical powers to the exigencies of the case is required. The objects to be attained are—1, extension of the knee; 2, the carrying forwards, and sometimes inwards, of the head of the tibia; 3, simultaneous inversion of the leg. I have ascertained, from repeated experience, that when attention has been directed to remove the abnormal flexion only, an increased displacement of the head of the tibia sometimes resulted. Thus, supposing the relation of the tibia and femur to have been, prior to the operation, as represented in the following diagram,

Fig. 6.

the effect of simply extending the limb will be to change the position of the bones thus,

Fig. 7.

the leg having been extended, but the displacement being undiminished. I have, in such a case, found the luxation complete, and the extremities of the bones as moveable as in a recent luxation from injury, the limb presenting to the eye of the almost affrighted attendant an appearance of greater deformity and weakness than before the commencement of his well-intentioned but injudicious management. In a common knee-extending apparatus no contrivance is present to facilitate the gliding of the bones into their proper position; on the contrary, the pressure necessarily exercised on the head of the femur, during the extension, forcibly urges the bones against each other. The method of obviating this imperfection in extending apparatus consists in employing the pelvis as a fixed point, and, by making extension from the ankle and foot, so to draw down the entire limb as will not only *extend* it, but elongate the ligaments of the joint, and other structures exterior to it, to such an extent as will enable the tibia to rise into its position. By this elongation the painful pressure of the femur and tibia against each other is completely avoided. When the ends of the bones are presumed to be held asunder, pressure on the upper and inner surface of the condyles, and on the upper and outer surface of the tibia and fibula, will essentially promote their reposition. The eversion

of the leg and foot is overcome by gradually drawing the malleoli inwardly, simultaneously with the more important process of reducing the luxation. The utmost caution is requisite during extension, to protect the point of the knee and heel from undue pressure, as excoriations or sloughs in these situations may entirely frustate the operation.

As after tenotomy, the limb will generally yield readily to a considerable extent, a common knee-extending instrument may be applied, during a week or a fortnight. This partial restoration will prepare the limb for the more difficult part of the process, for which I recommend the apparatus that is now before you (fig. 5, page 143).

On completing the extension and replacement of the knee, the joint is usually found to be very *stiff* in its new position; your next object will therefore be to restore its movements, which is often more difficult than the previous part of the treatment. Passive movements should be diligently practised, several times daily, or as often as the tender state of the articulation will permit; together with frictions, vapour baths, and the application, at intervals, of the extending apparatus, its action being reversed, in order to produce flexion. It occasionally happens that, with the utmost exertion, the knee cannot be bent to the extent of the previous abnormal flexion. Exercise may be permitted as soon as the limb is straightened; but, in the majority of instances, a firm mechanical support, as an ordinary *iron* on the outside of the limb, with a spring to assist the motion of the weak extensor muscles of the knee, will be necessary for weeks, or months, afterwards, according to the severity of the case. A laced knee-cap may be subsequently substituted for this support.

The average length of time occupied in the straightening and reduction of the displacement of articular surfaces after the operation, in adults, is two months; in children the success is more rapid. The process of restoring mobility, without which the individual continues to walk lamely, is seldom completed, in severe cases, within a period varying from three to six months, at the expiration of which the patient usually walks long distances very comfortably. If the articulation continue unusually tender, or the deformity have existed during many years, twelve months or more may elapse before the patient can be considered fully restored.

Should the case have been unusually complicated, as from adhesion of the patella, or should the permanently abnormal position of that bone on the upper and outer part of the external condyle, prevent action of the extensor muscles, the restoration of the knee may appear to be very imperfect. If it, however, be straight, and if the femur be fairly in apposition with the tibia, and the joint admit of motion in a range varying from 20° to 40°, the patient will be able, without artificial aid, to walk long distances, with perfect ease, and will be very grateful for the change in his condition.

No class of deformity better illustrates the advantage of the Stromeyerian operation for the relief of deformities; for, although the relief afforded by this method, when properly conducted, in cases of club-foot, may appear to be more complete (as not only the form but the mobility of the ankle, and the development of the muscles, will be more nearly perfect), yet the consideration of the comparatively greater importance and size of the knee-articulation, the circumstance that angular deformity of that joint necessarily impairs the functions of the corresponding ankle, together with the greater amount of lameness attendant on contracted knee than on club-foot, will suffice to show that *relief of knee-ankylosis* by *tenotomy* is as great an addition to the healing art as that of club-foot. *White-swelling*, for example, popularly regarded with horror, is, indeed, divested of its terrors, when it is considered how rarely, in the present day, owing to the improved mode of treating the disease in its earlier stages, amputation is adopted; and that—notwithstanding that this disease is peculiarly prone to terminate in deformity—it constitutes the most favourable form for dividing tendons, the subsequent mechanical treatment being more easy, and the restoration of the limb more complete, than in ankylosis from rheumatism, or from extensive suppuration from phlegmon.

Complete immobility (true ankylosis) *of the ankle* is uncommon, and need not long detain us. If the foot, although immoveable, be flexed to a right angle, so that the entire sole be applied to the ground, locomotion may be readily effected, and the assistance of art be unnecessary. Inconvenience is, however, sometimes experienced from the entire weight of the body being borne by the heel, and consequent transmission of shocks through the necessarily extended knee to the spinal column and its contents. This may be obviated by recommending the patient to wear a shoe with the heel sufficiently elevated to produce a small amount of flexion of the knee, and thus interrupt the continuous straight line through which the shock is communicated to the frame.

Deformity with Incomplete Ankylosis of Ankle.—The more frequent form of this affection results from strumous inflammation of the bones entering into the composition of the ankle-joint. You will find that in such cases the disease usually commenced in the lower extremity of the fibula and adjacent parts of the astragalus and os calcis, succeeded by suppuration and caries. The ankle-joint is weakened by the degeneration of the osseous and ligamentous structures which should support it in its external aspect, the patient is inclined to tread on the inner edge of the foot, and the peronei mus-

cles being henceforward relaxed, gradually contract and elevate the outer edge of the foot. The progress of the disease is usually slow, several months, or two or three years or more, elapsing before the ulcers completely heal. Cicatrisation is accompanied with the deposit of adventitious tissue beneath the skin, in the tract of the previous suppuration. This abnormal tissue constitutes firm bands of adhesions, connecting the eroded or carious extremity of the fibula with the astragalus and os calcis; and, by the occurrence of that contraction in its fibres which is peculiar to newly-formed tissue, the elevation of the outer edge of the foot is augmented. Increase of deformity rapidly follows the disturbance of the equilibrium of the muscles. The peronei so completely obtain the preponderance, that a considerable eversion of the toe is added to the elevation of the outer margin of the foot. The heel is elevated by contraction of the gastrocnemii. (See fig. *.)

Fig. *.

Deformity of ankle from articular disease, resembling talipes equino-valgus, characterised by elevation of the heel and eversion of the toes. Remediable by section of gastrocnemii and peronei.

Rheumatic inflammation of the foot may induce immobility and a similar contraction with deformity. But if the rheumatism do not especially affect the synovial membrane; but through the continuance of chronic inflammation in the muscles, tendons, ligaments, and fasciæ, on the posterior and internal aspect of the ankle, and particularly in the sole, if it induce so great atony in their fibres as will permit a sinking of the tarsal arch, a flat-foot results. Eversion of the toes, and stiffness of the tibio-tarsal articulation, causing the individual to walk on the inner margin of the foot, accompany the deformity.

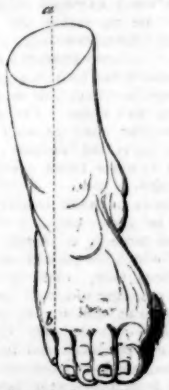
Deformity of the ankle, from disease of the leg bones, may, likewise, occur in the extended position, similar to talipes equinus. Here is the model of a foot which has been thus contracted for twenty years. (Fig. **.)

Fig. **.

Ankle deformity from articular disease, resembling talipes equinus. Remediable by section of the gastrocnemii, fascia, plantaris, and flexor longus pollicis.

Here is another model of deformity of foot from similar causes. In this instance the foot is not simply extended through contraction of the gastrocnemii, but the toes are inverted, from shortening of adductor muscles and of internal ligaments of ankle, assisted by contraction of cicatrices. Fig. ***.)

Fig. ***.



Deformity of ankle from disease of leg and tarsal bones (spurious talipes equino-varus). The perpendicular line *a b*, passing through the axis of the leg, shows the inward inclination of the anterior part of the foot, the little toe being alone situated on the outside of that line. The patient, therefore, chiefly walked on the ball of the little toe.

I have met with a perfectly similar deformity from fracture of fibula and injury to ankle-joint. You will experience no difficulty in distinguishing deformity of the ankle produced by disease or accident from similar changes in the form and position of the part, such as club-foot, and paralytic and spasmodic deformities. The history of the case, the nature of the stiffness, the dependence of the muscular contraction on causes primarily affecting the structures composing the articulation, will determine your diagnosis.

In each of the forms of deformity of the ankle that I have described, the rigidity of the articulation, whether resulting from adhesions within or without the capsular ligament, or from the consecutive muscular contraction, may be so considerable that scarcely any mobility of the bones entering into the composition of the articulation may be perceptible; thus, your opinion of the curability may depend on the detection of slight alternate tension and relaxation in the tendons of the contracted muscles whilst manipulating the limb.

The amount of lameness arising from these affections is often very considerable, and the inconvenience and pain experienced by the individual are usually greater than might be believed to exist. The circumstance which, in addition to the pain and discomfort attendant on locomotion with a stiff ankle-joint, occasions a ready application for relief is, the constant tendency, evinced in the severer forms, to increase the lameness and deformity consequent on the disturbance of the equilibrium of the muscles, and on the weight of the body being transmitted to the astragalus, whilst this bone is not situated horizontally beneath the axis of the limb, so as properly to communicate the weight, by means of the tarsal arch, to the ground. The portion of integument on which the greatest pressure in walking is exercised becomes excoriated or covered with painful corns, and sometimes severely inflamed.

Treatment.—The general treatment applicable to knee-ankylosis may be resorted to, with equal advantage, in slight cases in which the stiffness of ankle does not amount to apparent immobility. But if, from the duration of the affection, the intensity of contraction, or the failure of less active means, the case be judged irremediable without section of the more prominently contracted muscles, you may perform this operation; and it is fortunate that the treatment is liable to be attended with fewer inconveniences and interruptions than the similar affection of the knee-joint. Should contraction in the extended position of the foot exist subcutaneous section of the tendo-Achillis will, in most cases, sufficiently relax the articulation to permit its restoration; if the foot be rigidly fixed in the position of talipes equino-valgus (elevation of the heel with eversion), the peronei may require division; if in that of T. equino varus (elevation of heel with inversion), the posterior tibial tendon, flexor longus pollicis, and plantar fascia should be included in the operation. In slighter modifications of each of these varieties, you should be satisfied with severing the tendo-Achillis, and trust to mechanical extension for the elongation of the remaining contracted muscles and fasciæ.

I shall describe the apparatus applicable to these cases in a future lecture on some other deformities of an ankle.

ON A

NEW METHOD OF TESTING ARSENIC,

WITH REMARKS ON THE

METHOD OF REINSCH.

To the Editor of THE LANCET.

SIR,—While the name of M. Reinsch, and the reputation of his beautiful process for detecting arsenic, are yet fresh in the memories of your readers, will you permit me to draw their attention, and your own, to a new method of testing for this metal, which I have been engaged for several months in experimenting on, and in endeavouring to render as free from fallacy as possible.

It had struck me, almost immediately after the publication of Reinsch's method, that if copper chips and wire possessed the remarkable property, as they do, of separating metallic arsenic from a solution of an oxide of the poison, it might so happen that arseniuretted hydrogen gas would also deposit *its* arsenic upon copper, if allowed to pass over it; and to my equal surprise and pleasure I found that, by the application of a very gentle heat, such proved to be actually the fact. I had previously altered and modified Marsh's instrument in such a manner as to cause it to give me a constant and uniform stream of gas; removing the brass stop-cock, which is objectionable on account of its being readily contaminated after one experiment with gas, and its being also very difficult, if not impossible, ever to clean thoroughly; and substituting a glass one in its stead, ground into a tube bent like a syphon, and having three hollow globes on its long leg, and three of equal size on the shorter. All this seems complicated in description, but it is simple enough in reality, and the additional balls have the effect of enlarging the gas-room of the apparatus without materially increasing its bulk, and consequently the supply of gas is continuous and not intermittent, as in the original apparatus; and, indeed, in the little instrument I use it often continues for hours, so as thoroughly to exhaust all the arsenic in the liquid under observation. I thought I had now a good opportunity of combining

two or three different modes of testing in one apparatus; these are the arsenical stain, the reduction test, and the new mode which I will now very briefly describe.

Several pieces of clean, brightly polished copper wire of about an eighth of an inch in diameter, or some slips of thin copper ribbon (the latter are most convenient) are to be introduced into a tube of either German or English glass—it does not matter which, as no great heat is used in the process—which may be bent into an elbow at one end, and fitted with a perforated cork, if no better instrument be at hand, to a Florence flask at that end, the other being drawn out after the introduction of the copper into the tube to a capillary size. Some of the suspected matter, fluid or solid, with some pure zinc, pure sulphuric acid, and a little water (these latter must all be tested as to their freedom from arsenic previously), is then put into the flask so as to generate the arseniuretted hydrogen; this gas then passes over the surface of the copper wire, and may be burnt and tested in *that* way by the arsenical stain at the capillary extremity: a small spirit-lamp must, while the gas comes pouring over the copper, be held underneath it, so as to warm it for a few seconds, and almost instantly, if the arsenic be pretty abundant in the gas, the copper is seen to be frosted over with a coating of metallic arsenic, this deposit beginning at the end next the flask, and gradually extending until the whole surface is covered; the heat may be continued for a few minutes longer, and then withdrawn. If, however, the arsenic in the fluid being tested be small in quantity, then a minute or so may elapse before any indication of its presence is decisively given by the copper, but rarely longer. If it be requisite, and it is always expedient, to get as thick a crust as possible upon the copper, the heat must be continued for four or five minutes more; but if kept up after this, as no more copper surface remains exposed to the gas, and as probably the tube has become nearly red hot, then the arsenic begins to be deposited on the sides of the glass tube. It is rather singular that as long as any portion of the copper remains uncovered by this deposit, so long will the sides of the tube be perfectly clean and unstained. The copper may then be withdrawn, and it will be found to be coated over with a beautiful silvery crust of arsenic; this soon loses its lustre, and the wire appears as if it had been transformed into zinc or lead. It is to be noticed that every portion of its surface is covered with this crust, both that more directly exposed to the gas, and also that next the tubing. If antimony have been present instead of arsenic, the wire is blue, often iridescent, and is only coated on the parts directly exposed to the contact of the gas; a much more intense heat is requisite to get a deposit at all, and the sides of the tube are invariably stained owing to this increase in the temperature. In two words, antimony requires more heat to decompose it than arsenic, when in combination with hydrogen gas, and this explains most of these phenomena, and, indeed, is of itself by no means an unimportant feature in the means of distinguishing the one from the other. The copper wire or ribbon is now to be cut into small pieces a sixth or a fourth of an inch in length, put into a tube drawn out at one end fine so as to prevent their slipping through, and heated gently over a small flame until the chips are nearly of a cherry-red heat.

1. If the deposit be arsenic, long before all the crust is oxidised off, evidence, beautifully decisive and clear, is obtained of its presence in the deposition of the brilliant, glistening, transparent little crystals of arsenious acid, in a ring, presenting a sharply-defined margin towards the copper chips, and shading gradually off towards the open larger end of the tube. A Stanhope lens is sufficient to complete the proof; the ring, which is distinctly—if I may use the word, unmistakeably—crystalline to the unassisted eye, is found on examination to consist of, octahedral crystals, the triangular facets being presented to the eye; sometimes they appear tetrahedral, probably from the octahedron lying on its edge, and presenting the faces of the two opposed triangles towards the observer. I feel quite certain that any one who had but once seen them could never again either forget or mistake anything else for them. After their shape has been clearly made out, the farther proofs of their nature may be obtained, as Dr. A. T. Thomson and Dr. Christison have shown, by dissolving out the arsenious acid with a little water, containing a few drops of nitric acid or ammonia, dividing the solution into three parts, sending a little stream of sulphuretted hydrogen gas through one, and adding the solution of the ammonio-nitrate of silver and of the ammonio-sulphate of copper to the two others; and now the orange, the lemon-yellow, and the grass-green precipitates, decide the matter.

2. Suppose, however, the deposit on the copper is antimony, it must be treated in just the same manner, and it will be noticed that the heat requisite for obtaining any oxide is much greater than that in the arsenical case; that, instead of the crystalline ring, a film of a white, amorphous, uncrystallised powder is obtained, sometimes forming an indistinct ring, never having so sharp a margin as the arsenical, and sometimes left like a delicate white dust along the length of the tube: altogether, the result of the process is as remarkably indefinite and uncertain in this case as in the former it is definite and decisive.

You will, of course perceive Sir, that this oxidation test is that which was proposed originally, I believe, by the late Dr. Turner,

so that all which is mine in the process is the first part of it—the method of getting the deposit. The whole matter appears infinitely more complex and difficult on paper than it is in operation; and, indeed, the chief beauty of the whole consists in its simplicity, its certainty, and the facility with which the most inexpert toxicologist may obtain his evidence. I at once admit, and, indeed, those who have troubled themselves so far as to wade to this depth in this account will almost anticipate my avowal, that inasmuch as Reinsch's test has been fairly proved to be somewhat—although, I think, not as much as is generally stated—*more* delicate than Marsh's, so it of necessity follows that it must be more delicate than mine, as both my test and Marsh's depend for their essence upon the generation of arseniuretted hydrogen gas.

The reasons which, perhaps, I may urge why this method may be adopted with as much confidence, if not more, than that of Reinsch, are the following:—The arsenical deposit is more decisive in its aspect, and more readily and at once distinguished from that of antimony by its silvery lustre, and by the blue, sometimes powdery aspect of the latter; and the very process of getting the deposit gives some help to the tester to diagnose his poison, from the slight heat requisite to obtain it from arsenic, and the greater from antimony, the tube being clean with arsenic, stained with antimony, and the deposit being most frequently, if not always, on every portion of the surface with arsenic, and chiefly on the exposed surfaces only, with antimony. For all practical purposes it is as delicate, and, I believe, is not affected, at any rate to any confusing degree, by the other metals, tin, bismuth, &c., which affect M. Reinsch's: on this, however, I do not lay much stress, as it requires more investigation than I have as yet been able to give to it.

As to the question of its delicacy, I have little farther to state than this, that it seems to me that this point is overstrained. I have examined a great many reports of cases of poisoning by arsenic, and from all I can collect from them I cannot but infer that it seems improbable, at the least, that the poison should have been the cause of death, if it be not detectable from examination of either the vomited matters, the lining of the alimentary canal, the blood, and particularly the urine, if any can be obtained, by the generation of the arseniuretted hydrogen. So strong seems the affinity of arsenic to hydrogen to me, that the quantity must be almost infinitesimal if it will not show itself, and, if it *do*, then my test will find it out; so that, unless a man be poisoned by some homœopathic murderer, we shall have evidence enough to prove the existence of the arsenic by this method alone. I have particularised the urine, as I do not find in the cases I have examined any mention of *its* analysis; and Orfila has shown that the kidneys do their utmost when arsenic is in the system to get it out again, unless its narcotic effects shall have at all paralysed them: in some future case it might be worth while attending to this point.

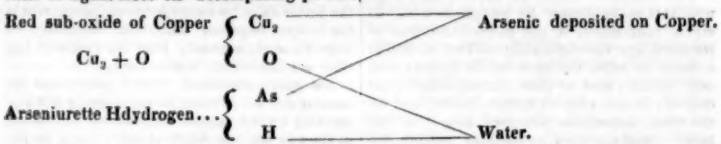
I may mention, before proceeding further, that if the quantity of arsenic be very minute, it is advisable to keep up a constant gentle heat, confining portion after portion of the gas over the wire by closing the end of the tube, until all its arsenic has been deposited.

Thus far, perhaps, it might have been foreseen that arsenic *would* be deposited upon copper when heat was applied, as we find it deposited on the sides of the tube under apparently similar circumstances; but it is to be observed, first, that the heat requisite for the decomposition of the gas must in the latter case be carried to redness, while the copper requires much less intensity of degree; and, secondly, that the sides of the tube are left clean when copper is heated in the gas, facts which seem to prove that the arsenic has an affinity in the copper not depending on the chemical alteration produced in the gas by the heat alone. Even so far there is nothing either very unusual or very extraordinary; but I now come to a fact which does seem both new and singular; it is this. I have found that when copper wire or ribbon is exposed *per se* unheated, and unaided in any other way, to a stream of arseniuretted hydrogen, it possesses the property of decomposing it, and causing the arsenic to be left as a metallic crust just similar to that obtained in the former experiments upon its surface. This decomposition is rather slow; perhaps it may take twenty minutes or half an hour before all the surface is covered, but it is not the less certain. Let it be now tested by oxidation; cut the wire into small chips, and very carefully dry it, so as to drive off some moisture on its surface, as this, if not driven away, rises with the crystals of arsenious acid, and is apt to dissolve off their angles, making them look like truncated cylinders. Heat them now as before, and as before will be seen that decisive and indubitable proof of the nature of the coating, the little crystals of arsenious acid, in a delicate zone on the tube.

This is not all, however. This deposit of arsenic will not take place (as far as I have been able to make out), unless the *surface* of the copper be in a particular condition; and when I first distinctly proved this, I was congratulating myself on my good fortune in being able to add another to the list of katalytic decompositions. However, my self-gratulation died away upon a little more reflection, and the apparently reasonable explanation of this singular circumstance will be given immediately. The particular con-

dition of the surface to which I have alluded is rather remarkable. If the copper be perfectly bright and clean, I have never succeeded in getting any deposit on it when exposed without heat to the gas; but if, however, the copper be heated in a fire until it is dull red, and then suddenly plunged into cold water, although this last is not essential but simply convenient for purity's sake, then it *will* decompose the gas. If this has been properly managed, and it requires very little attention to do so, the surface of the copper should be of a maroon or reddish brown colour; this seems to arise from superficial oxidation of the metal taking place whilst it is passed from the fire to the water. It must be observed that if the copper be heated to bright redness and then suddenly cooled, this oxide falls off, or the metal is technically said to "scale;" and *then* it again loses its decomposing power.

All these circumstances go to prove that it is necessary to have a surface not of copper alone, but of its oxide; and I have found in a vast number of experiments that this is really the case; and I have by me now many specimens where the copper has partly scaled and part not; and on the parts where the scale has not fallen off arsenic is deposited, while on the clean surface there is not a trace of it. This would seem at once to point to its own explanation, and that which seems most probable and rational to me is this: we have an oxide of copper, over which a gas containing arsenic and hydrogen is passed; the hydrogen I suppose to combine with the oxygen of the oxide, and to form water, while the arsenic is deposited on the copper in its metallic state. This may appear clearer by the following diagram:—



In support of this I may add that I have noticed the deposition of minute globules or points of water on the surface of the copper after it had been exposed to the gas, although I had taken means to dry the gas previously.

I have not heard of any explanation of the method in which arsenic is deposited in M. Reinsch's test; may I venture to offer the following rationale:—

We have hydrochloric acid and arsenious acid in solution in water, with slips of metallic copper; this solution, heated to 212°, a chloride of copper is formed, and the hydrogen resulting from the decomposition may, I think, form arseniuretted hydrogen, from decomposition of the arsenious acid, the oxygen of which may unite with the proportion of hydrogen and form water, while the arsenic is deposited as a metallic coating upon the chloride of copper, and the hydrogen again liberated to decompose a fresh portion of arsenious acid. If this explanation, complicated, as it appears, be correct, then, after all, Reinsch's test is but a modification of Marsh's; but I fear some more learned chemist than myself will unpick this theory of mine. I hope, however, some better one may be found in its stead. *Surface* appears to have no effect on arsenious acid in solution; that is to say, no arsenic is deposited on either a bright or an oxidated surface; and, indeed, if my explanation be correct, why should it, so long as we withhold the materials for the supply of the hydrogen, which appears the *sine quâ non* of the experiment.

I must, however, stop theorising further, as a young chemist in so doing always walks over hidden mines, and does not find out his error until he is blown up.

Do not, however, let me be misunderstood about this last process of mine. I do not urge *it* as a test for arsenic; it is too slow in operation, and is liable to be influenced by many trifling circumstances which might interfere with its indications. I have merely alluded to it in this paper as I believe it to be a novelty in chemistry, and one that at first sight appears rather startling. I wish I could explain away all apparently katalytic actions as readily as I hope this has been explained. It may be that some equally simple causes influence them could they but be found out. I first found that it required the surface to be modified by accidentally putting into the gas a piece of wire which I had heated, and then cooled in water just to cleanse it from some impurity. I hope there may not have crept into this account any very serious mistake, as I have made some hundreds of experiments on the subject, fearing my own manipulation for some time, until at last I could not but be convinced that the facts are really those which I have occupied so much—too much, indeed—of your Journal in detailing. I sincerely trust, however, some more expert and experienced toxicologist than myself will take up the matter, if it be worth the trouble, and then decide the accuracy or the falsehood of my experiments.

Before closing, I may add that I had the pleasure of showing the results of my first process to that "Prince de l'Appareil du Marsh," as the French call him, Orfila, at Paris, who, while he smiled at the compli-

cated aspect of a sketch of my modification of Marsh's instrument, seemed pleased with the results of the test. Apologising to you, Sir, and to your readers for so long encroaching on your time and patience and on theirs, I remain, Sir, your reader,

ROBERT ELLIS.

University College, Oct. 25, 1843.

RUPTURE OF THE HEART, AND SUDDEN DEATH.

To the Editor of THE LANCET.

SIR,—I send you the particulars of a case of rupture of the heart, which, I think, will be worth communicating, as examples of it occur rarely, and may then become the subject of medico-legal inquiry, as in this instance:—Mrs. Keele, aged 53 years, the wife of a farmer's labourer, was taken ill suddenly on the morning of the 20th ult., and died in the course of half an hour. On my arrival the body was still warm, but afforded no indication of the cause of death, I therefore obtained an order from the coroner for a post-mortem examination. In answer to my inquiries the husband of the deceased stated that she had been in very good health, and only the day before had walked five miles with a burthen, but she had complained frequently of a bronchocele, which embarrassed her breathing at times, and to which he attributed her death. In December, 1842, she was ill with fever, and had an inflammation of the chest (but no decided information can be obtained on this point).

At half-past three, a.m., of the abovementioned day she was awakened by severe cough and difficulty of breathing. After making signs of great suffering about the throat she "reared up" suddenly, and fell forward on the bed; when he obtained assistance she was quite dead. Two hours after death I found that the body had been laid out; it was that of a person of middle stature and spare conformation; the limbs were relaxed, and the face and general surface very pallid. There was a goitrous tumour, which was not tense, nor of a size to justify the inference of death from that cause.

Autopsy, Thirty Hours after Death.—The body was now quite rigid and much congested in the depending parts; there was no œdema of the lower extremities. On the sternum being raised the lungs collapsed moderately; the pericardium was quite flaccid, and not at all encumbered with fat; the heart was found occupying its normal position, but obviously enlarged and much charged with fat. The right ventricle was collapsed, and on the middle of its anterior surface, close to the septum, I observed a depression which proved to be a perforation into its cavity, large enough to admit a goose quill, and showing one of the fleshy columns through it. About an ounce and a half of fluid blood was effused into the pericardium; the inner surface of this membrane was marked by two small white patches of lymph anteriorly, but was otherwise healthy. The heart was removed, the usual quantity of dark blood flowing from the divided vessels: its *weight* was eleven ounces (avoirdupois). There was a thick deposit of yellow fat under the serous covering, most abundant at the base and in the course of the coronary arteries; also on the anterior surface. The superficial vessels were much injected. On the cavities being laid open, the muscular tissue appeared somewhat pale and softened, its fibres being intermixed with fat. The right ventricle rather dilated, and its walls much thinned, so that near the point of rupture their thickness did not exceed half a line, and here the accumulation of fat was greatest. The opening was of irregular shape, with a torn edge and its greatest diameter transverse, and was situated two lines from the septum. The endocardium and valves were not at all diseased.

The left ventricle, which contained no coagula, was ten lines thick in the middle of its wall; the lining membrane of the aorta in its commencement was dyed of a bright red colour, and marked with numerous white spots. The aortic valves presented the same appearances, and there were several patches of redness on the musculi papillaris.

The lungs were everywhere adherent to the costal pleura inferiorly; the surface of a deep red, and near the anterior margins studded with emphysematous vesicles. The tissue of the lung was highly congested, but crepitated well on pressure.

The bronchi contained much mucus, and their mucous membrane was injected, as well as that of the trachea. The bronchial glands were enlarged, and many of them filled with calcareous matter.

The bronchocele consisted chiefly of enlargement of the central portion of the thyroid body, and was the size of an orange. A section of it exhibited the usual cellular structure, with a fibrinous centre. The other cavities were not examined.

The only comment that I have to make on the case is that a very small quantity of blood was effused from a considerable breach in the ventricle, whence I conclude that death must have been instantaneous from the heart's action being suspended by pressure of the sudden effusion. I remain, Sir, your obedient servant,

HENRY T. TAYLOR.

Guildford, Oct. 30, 1843.

IN February and March, 1843, as many as 11,311 individuals were received into the various hospital in Vienna and its neighbourhood, of whom 5650 were afterwards discharged cured, and 670 died before April arrived.

GENERAL HOSPITAL, CHELTENHAM.

To the Editor of THE LANCET.

SIR,—I should feel obliged by the insertion of the following case in the pages of your excellent periodical, the more particularly from its having been made the topic of much discussion in consequence of the propriety of the operation detailed having been called in question. I remain, Sir, your most obedient servant,

WM. P. BROOKES, M.D., M.R.C.S.L.,
Resident Surgeon, &c.

Oct. 16, 1843.

STRANGULATED FEMORAL HERNIA OF SEVEN DAYS' DURATION.

Sarah Nicholls, aged 60, labourer, of a spare habit of body, of the parish of Overbury, on Saturday, August 19, 1843, whilst employed in the corn-fields, she found, for the first time, what she described as a swelling in the left groin. From its not giving her much pain she did not pay any particular attention to it for a day or two, but could not continue her usual occupation. The symptoms continued daily increasing, and on the 25th of August (seven days from its first descent) her friends sent for Mr. Tate, surgeon, Tewkesbury, who immediately ordered her to be removed to this institution (the Cheltenham Hospital) for the operation to be performed.

On her admission, I found on examination a swelling, of the size of a walnut (femoral hernia), in the left groin; the skin covering it was much inflamed, and had become of quite a pink colour. The woman said that her bowels had not been opened for a week; she was vomiting green bilious fluid, which she told me had been the case incessantly, both night and day, ever since the hernia came down. The bowels were tympanitic and excessively tender on pressure; tongue covered with a dark-brown crust, and exceedingly dry; countenance cadaverous and anxious; pulse small and wiry. I ordered her to be immediately placed in a warm bath, and on her removal, finding I could not reduce the hernia, I had recourse to ice, but, on the application of both these means failing, I deemed it prudent not to carry the taxis any farther, and accordingly sent for the hospital surgeon, Mr. Eves, who immediately proceeded to operate. The operation was performed in the usual manner, with one exception, viz., that of dividing the stricture before opening the sac. On the sac being opened the intestine was found of a dark-chocolate colour, and had no omentum with it. The wound being dressed the patient (who was exceedingly exhausted) was placed in bed, and no medicine given her, but the bowels acted copiously in a few hours after the operation To take fifteen drops of sedative solution of opium at night.

26. Complains of sickness; tongue furred; pulse fuller, and less wiry; bowels copiously opened, but still tympanitic. To take an anodyne draught at night. An ounce of effervescing mixture every four hours. Two drachms of brandy in gruel occasionally.

27. Bowels not opened, less tympanitic; slight pain on pressure; pulse natural; sleeps badly; sickness continues. Continue the effervescing mixture.

℞ *Chloride of mercury*, six grains;
Powdered opium, a grain. Mix into six pills; one to be taken every six hours. Half an ounce of wine occasionally.

28. Bowels not opened since Aug. 26th; tongue brown and furred; pulse 80; slept well; vomited a good deal of green fluid during the night and yesterday, but it has ceased to-day. Omit pill and mixture.

℞ *Sulphate of magnesia*, an ounce;
Tincture of henbane, two drachms;
Camphor mixture, eight ounces. Mix. Two tablespoonfuls to be taken every four hours until the bowels be moved.

30. Bowels not yet opened; is slightly delirious; no pain of the abdomen on pressure; pulse 80; tongue furred. The wound looks very well.

℞ *Castor oil*, half an ounce. To be taken every four hours, until the bowels be moved.

Seven, p.m. After taking two doses of the oil the bowels were freely opened.

31. Bowels well opened; going on well in every respect, but complains of great prostration of strength. A glass of port wine daily.

℞ *Compound tincture of bark*, half an ounce;
Sesquicarbonate of ammonia, twenty-four grains;
Infusion of bark, eight ounces. Mix. Two tablespoonfuls to be taken three times daily.

She continued from this time to go on well, and was discharged, cured, on Oct. 2, 1843.

Remarks.—I imagine, after reading the above case, that but few surgeons could doubt the propriety of the operation being immediately performed. We are well aware that the closeness of the stricture in femoral hernia particularly renders the chance of reduction uncertain by any other means than that of the knife, and the great pressure which the parts sustain in these cases must, at the least, render delay very hazardous. The number of days during which strangulation had existed, and the descent of the intestine being of a recent occurrence (for she had never observed it before the day in the corn-field), made the case most urgent, and, in fact, from all her symptoms on admission

into the hospital, it was deemed almost hopeless; yet the only chance left to the poor creature was to operate, and to this she most willingly consented.

The division of the stricture before the opening of the sac could hardly be deemed a peculiarity worthy of notice; for we have the authority of many eminent surgeons to return the intestine into the abdomen without opening the sac.

Slight stimulants were early administered to the patient, with supporting diet. This, together with the avoiding of all active aperients, is the after-treatment generally adopted by Mr. Eves in cases where no inflammatory symptoms are present, and the patient is advanced in years, and has the vital energies much depressed,—calomel and opium, with fomentations, being used to allay irritation, and to prevent the development of inflammatory action. This treatment has been very successful in the Cheltenham Hospital, and has been previously noticed in THE LANCET.

W. P. BROOKES.

SINGULAR CASE OF

MALFORMATION OF THE SEXUAL ORGANS,

WITH ABSENCE OF THE NORMAL URETHRA.

BY

EDWARD SMITH, M.B., Birmingham.

THE following case, forwarded for insertion in THE LANCET, will, it is hoped, be thought worthy of the attention of the profession:—

On September 13th, 1843, Mrs. Tinley, of Constitution-hill, was delivered of an infant having malformation of the sexual organs. The perineum was about one inch and a half in length, having the precise characters of that part in the male. At its anterior termination, and within five lines of the arch of the pubis, was a triangular-shaped opening, the apex being above, and in front. Through that opening the urine was emitted; and along the canal, terminating there, a bent probe could be easily passed into the bladder. There could, consequently, be no doubt of its being the urethra.

In the central part of the perineum, and about midway betwixt the urethral opening and the anus, a slightly elongated depression might be perceived, having a faint blue tint shining through the skin. This depression induced us to think that it might be the extremity of a vagina; it was not, however, of sufficient weight materially to affect the opinion.

In front of the pubis was an elongated cylindrical body, having the *precise* characters in position, form, and resistance, of a penis with the prepuce withdrawn. A substance, perfectly similar to the glans penis, was very evident, having in its centre an elongated fissure at the point where the urethra usually terminates, but no opening of the urethra could be detected in it. This penis was nearly one inch in length, and was curved downwards, being connected by its frænum preputii with the central line of the perineum. This connection was immediately above the urethral opening, and with this opening the fissure in the glans penis would have been continuous were it not for the connecting band above mentioned. On either side of this penis an oval, flattened, sac-like body was placed, measuring twelve lines in length and eight lines in breadth. In form and position, this body, as well as in the colour and striated appearance of the integument by which it was covered, *very* much resembled the scrotum. It differed from that organ, however, in the two sides being separate and distinct. In the space betwixt these two sides of a scrotum the penis was situated. Neither testes nor spermatic cord could be felt.

It now became difficult to ascertain whether these organs belonged to a male or a female child. The appearance was certainly that of a male, since we had a body much resembling a penis—two organs analogous to the scrotum—and an urethral opening at the base of the penis. The non-perforated character of the penis did not militate against such a conclusion, since such a circumstance has been occasionally found in individuals whose sex was certainly the male. The same remark also applies to the want of testes in the scrotum, inasmuch as many individuals have reached manhood before the testes had gained their proper position. The opening of the urethra, although abnormal, seemed to assist us in our conclusion, both since its *absolute* position was unlike that of the female, and from its *relative* place with regard to the base of the penis. We could not consider it to be a female, because we had not the external fissure bounded by the labia, nor the nymphæ, with any of the more internal of the external organs of generation. The scrotum-like bodies might be transformed labia, and the penis-like body might be an enlarged clitoris, but the *tout ensemble* was that of a male child.

On the following day Mr. Green and Mr. Thos. Chavasse, two highly talented surgeons, examined the child, and unhesitatingly pronounced it to be *a male*.

During the succeeding five or six weeks the infant was very restless and irritable, and became gradually emaciated, and a considerable quantity of opake mucus was discharged by the urethral opening. On Octo-

ber 30th it died, and an examination of the body was made on the same day, when the following unexpected and unusual circumstances presented themselves.

A normal and well-developed *uterus, with its Fallopian tubes and ovaries*, was present. The canal leading from the external opening was *the vagina*, about two lines in diameter, *perfectly cylindrical and patent*. There was *no urethra*, but the *neck of the bladder was inserted into the vagina*. That this canal was the vagina, although not perfectly normal, is proved from these circumstances,—First, the direct course of the canal was *to the uterus*, the bladder being connected with it at an obtuse angle. Secondly, the structure of its walls was that of the vagina, consisting of a mucous membrane, a dense layer of coloured fibrous tissue, and some condensed cellular membrane. This structure was continued *beyond* the insertion of the bladder into the vagina, even to the neck of the uterus. The anormal condition of this vagina was its not terminating by a cylindrical opening, around which a fibrous layer might be located. The above-mentioned structure, also, did not proceed to the termination of the canal, but only its most internal part, about twelve lines. The more external eight lines were composed of mucous membrane and cellular structure only; but although the lower part of this canal bore some resemblance to the urethra, yet the circumstances above named will probably prove it to have been the vagina.

Neither testes nor spermatic cord could be discovered. The labia consisted of a portion of integument and cellular tissue folded upon itself, the two layers being very readily separated from each other.

This case appears to be interesting for three reasons:—

First. From the great similarity of the external parts to the male organs of generation, which was infinitely greater than is usually observed in anormal conditions of the female organs.

Secondly. From the canal consisting of vaginal tissue, with an urethral termination, the external opening also being anormal; and more especially from the direct insertion of the bladder into the vagina, causing the absence of the urethra.

Thirdly. It is illustrative of the remark made by Geoffroy St. Hilaire, that the organs of both sexes are originally formed on the same plan. It also shows how purely speculative must be the opinion of medical men in such anormal cases.

Birmingham, Nov. 1, 1843.

Fissures of the Anus.—For these MM. Bretonneau and Trousseau recommend enemata of four scruples of extract of rhatany to half a pint of water to be thrown up the rectum once or twice daily. In a few days the pain ceases, and the fissure heals.

DOUBLE BRAIN.

To the Editor.—Sir: On October 17th my attendance was requested on Mary Nelson, ætat, 32, who on that day was seized with labour-pains of her seventh child. On arriving at the house, and making an examination, I found the os uteri fully dilated, and the pains strong and constant. The vertex presented, with the face towards the left ilium. Upon the head descending into the cavity of the pelvis I became aware of the existence of a tumour, (the nature of which I could not at first distinguish. The pains, however, soon expelled the child, when I found the tumour to be attached to the occipital bone, and of the size of a large orange, of a soft nature, and totally devoid of hair, although hair was plentiful upon the head. In connection with this the child had spina bifida of the third, fourth, and fifth dorsal vertebræ, and the right foot was clubbed. The other organs were normal. The child was seized with convulsions immediately after its birth. These continued, at intervals, until its death, which took place on the following evening. Permission being given, I opened the tumour on the morning afterwards, and found it to contain a perfectly-developed brain, surrounded by its ordinary membranes, and connected with the posterior part of the cerebrum within the skull, through a circular opening of about an inch in diameter, just above the occipital protuberance, and having, also, a distinct connection with the spinal cord, the nerves being given off in the usual manner. I am, Sir, yours respectfully,

E. J. Barker.

Northwich, Nov. 1, 1843.

THE STRUCTURE OF TENDON.

THE QUESTION OF THE UNION OF TENDON AND MUSCLE.

To the Editor of The Lancet.

Sir,—The question has long been agitated in what manner the union between tendon and muscle is effected, and notwithstanding the valuable researches that have been made of late years, we do not yet seem to have come to a satisfactory conclusion on the subject. This appears, in great measure, to have arisen from the fact that the inquiry has been directed rather to the immediate union of the two structures than to that of their elementary parts.

Each primitive fasciculus of muscular fibrillæ has been found by Mr. Bowman to be enclosed in a delicate sheath, which he has named the *sarcolemma*, separate and distinct from that which surrounds the larger fasciculi and the entire muscle. Now, the cellular tissue from which the external sheath is derived is represented as being continuous throughout the entire body; and from the

simple circumstance of the sarcolemma being distinct from the common sheath it has been supposed that its nature differs likewise; but this inference has been drawn, too hastily, from an observation made on the more central portion of the fibre. If we examine the common sheath of the muscle itself we find it generally distinct from the surrounding cellular tissue, and presenting a clean, polished surface,—in no sensible respect differing from the sarcolemma, with the exception of its greater density. We may thus regard the common sheath as a portion of the general cellular tissue, isolated from the rest, excepting at its extremities, and sending down processes from its inner surface between the fasciculi; and we may regard the sarcolemma as a portion similarly isolated from the general sheath. Whether it give similar processes to surround the individual fibrillæ remains to be considered.

The elements of cellular tissue may be said to be two, viz., the ordinary filamentous structure derived from the original cells and cytoblastema, and the yellow fibre, &c., formed by the elongation and interunion of the nuclei. The first are rendered indistinct by acetic acid, which thus affords a more ready view of the nuclear portion. Now, in the sarcolemma, we must bear in mind its extreme tenuity, caused by the matter of a few cells being spread out over a large surface. Hence, also, the fewness of the nuclei that are found in it. In speaking of the nuclei of this membrane I refer to the muscular corpuscles of Mr. Bowman, and I should, therefore, state my reasons for altering their designation. I believe them to be identical in their nature with the nuclei of ordinary cellular tissue, from the evident ratio that they bear to the tenuity of the membrane, and consequently the small quantity of cell-matter required in its formation, and from their behaviour under chemical reaction. I would also refer them to the fibre sheath rather than to the proper sarcous material, because of the regularity of their disposition on the surface of fibre. But other similar corpuscles are observed disseminated in the mass, which at first sight do not present this regular disposition.—What does this go to prove? That these bodies belong to the proper substance of the muscle? No. This appearance would rather suggest to us the existence of sarcolemmatous processes corresponding with those that are sent down from the general sheath, between the larger fasciculi; and, by carefully varying the focus of the microscope, I think I can perceive an arrangement which would support my view.

But to proceed with our original question. We are well acquainted with the continuity and identity of the fibrous and cellular tissues, they differing chiefly in this, namely, that in the former the nuclear filamentous portion is the more developed. Now, let us consider the filaments of the tendon as being continuous with the sheaths, or, rather, closed sacs of the muscle and its primitive fasciculi. In those of the latter (the sarcolemmata) we find the nuclei free and ununited from their remoteness; and we can trace (at the extremities of the organ) the gradual condensation of the tissue, and the consequent approximation of its fibres, where they unite, after the ordinary mode of cellular structure, until we arrive at the tendon itself. The transition, although rapid, is so gradual that it is impossible to fix the limits of any one region of condensation.

With this view of the constitution of tendon it is easy to see in what manner it is influenced by the contraction of its muscle. And I may add, that it is supported by an observation of M. Lauth, showing the laxness of the sarcolemma when the muscle is in a state of contraction. I have the honour, Sir, to remain your most obedient servant,

E. C.

Dorset-street, Portman square,
Nov. 1, 1843.

MEDICAL INTERFERENCE IN PROTRACTED LABOURS.

To the Editor.—Sir: In THE LANCET for Sept. 30, 1843 (page 941), a case of laceration of the perineum during labour is related by Mr. Robinson, of Lewes. The case is not rendered sufficiently complete to enable a true estimate of its real nature to be formed. We are not informed of the patient's age, nor of the state of her health previously to the commencement of labour, nor at what period the membranes broke; neither are we told when the labour terminated. It is stated that on examination the parts appeared to be unusually small, and the perineum peculiarly rigid and unyielding; and that previously to marriage an abscess had formed in the parts, the cicatrix occasioned by the opening of that abscess being considered to be the cause of the obstruction or rigidity of the perineum. Hot fomentations, with a view to relax the rigidity of the parts, seem to have been the only means used.

Independently of the severe and protracted suffering which this woman underwent before the birth of her child, it appears that her condition afterwards was serious and her recovery tedious, even under judicious treatment. A case similar to this one is reported in THE LANCET for February 4, 1843, on which case I made some observations in a subsequent number of THE LANCET, and as those observations had for their object the safe and speedy relief of such cases, I beg to refer Mr. Robinson to them, with very little doubt that he would, by careful trial of the means advised, save himself much time and anxiety, and his patient much suffering.

It is truly pitiable to see works still issuing from the press professing to guide the obstetrician, advising as little interference as possible with nature in the usual forms of protracted and tedious labours. Their authors seem never to consider that, although nature, in her normal condition, undeviatingly performs her operations well, yet that in her abnormal or morbid states she is uniformly defective in accomplishing her natural processes, the degree of this deficiency being always proportioned to the extent of the disturbing causes.

The accoucheur should immediately ascertain the nature of the obstructing causes, and the means suited for their removal, and then, without waiting until nature is exhausted by her exertions to overcome resistances that are beyond her powers, he will instantly resort to those means which are calculated to subdue the abnormal opposition, as when the operations of nature are thus unopposed delivery will be speedily effected. So far as I have observed, these obstructing causes to delivery may, by careful observation, generally be easily recognised, and the remedial means as readily known and applied. The advocates for non-interference might as well argue that as, in many complaints, patients recover without medical treatment, nature should always be left to her own resources so long as no immediate danger appears. Yet in this way many lives must be lost which might be saved, and many recoveries grievously protracted. The same reasoning applies to parturient women in protracted labours, where, without removing the causes of delay, they must, for a great number of hours, suffer unnecessarily, and recover tediously if surviving; but not unfrequently both mother and child are lost. In my opinion such views and recommendations are most injurious, for when explained to the sufferers by their medical attendant, as the mode of practice pursued by men of great experience, the patient submits, and the practitioner considers that he is doing "all that duty demands of him." Yet were general practitioners oftener left to draw from the resources of their own minds they would (in many instances at least) conduct their cases more in conformity with the requirements of nature. I am, Sir, your obedient servant,

JOHN CRAIG.

Paisley, Oct. 31, 1843.

STERILITY.—If the mule be unproductive it is (in the female) that the animal is not liable to periods of heat, as are all other female quadrupeds, consequent on the ovaries being destitute of Graafian follicles. The male of the same kind is equally incapable of propagation, because the semen is destitute of spermatic animalcules, "which, apparently, constitute the fecundating principle in all animals."—*Raciborski.*

THE LANCET.

London, Saturday, November 11, 1843.

THE virtues of *simple arithmetic*—which, when occupied in the deduction of medical facts, is, by general consent, called *numbering*—have no operation more important than that of calculating the efficacy of *remedies*, for *numbering* is the only method by which their qualities can be satisfactorily proved, though almost wholly neglected by us, very many medicines which are in daily use being indebted for their character simply to hearsay, and not to that of effectual test. Accordingly, the most diverse opinions prevail, even among intelligent practitioners, with regard to the pretensions of numerous presumed therapeutic agents. One, for example, says that he has the greatest confidence in the alterative virtues of *sarsaparilla;* another, with equal opportunities of observation, declares his belief that its qualities are precisely equivalent to those of chopped hay. Some experimenters will affirm that *iodide of potassium*, given in doses of more than eight or ten grains, will act as an irritant, producing diarrhœa, vomiting, and other disagreeable effects; while certain inquirers, on the other hand, assert that six drachms of that substance may be given daily, in divided doses, for many weeks, and even half an ounce of it at a single dose, without inconvenience to the patient.

It is quite clear that—the constitution and condition of the patients being analogous—one or other of these statements is egregiously erroneous, although both profess to be founded on personal observation. It is needless to multiply instances. There are, in daily use, a great number of alleged medicinal substances, with reference to which it is disputed whether they have any operation at all, or, admitting that they have *some*, what that operation is, and under what circumstances it occurs. But to ascertain whether a given substance be active or inert, in relation to the animal economy, and,

—if it have an appreciable action,—to determine what that action is, are points of inquiry within the compass of every individual who is endowed with common sense, and willing to incur the trouble of the investigation. Hence the fact that if any uncertainty exist on such questions it is discreditable to medical science, their solution being mere matters of arithmetic. But cyphering seems as irksome to doctors as to schoolboys, the greater part of us preferring to exercise our faith or our fancy to using our tablets. The expression "cæteris paribus" is common enough in medical language, but that distribution of objects which is necessary to render the phrase applicable, is lamentably rare in medical inquiries. In no other department of human knowledge are to be found such *discrepancies* of opinion as to what ought not to be matter of *opinion* at all, but *matter of fact;* nor is it surprising that sound-headed men of other professions should often turn from medicine with incredulity and contempt, as from a science that is without principles, and an art without efficacy.

The numerical method may be applied to therapeutic operations with greater facility than to most other branches of medical inquiry, because we have here the advantage of knowing the nature and proportion of at least one of the agents that are concerned in the actions under investigation, namely, the medicine itself; whereas in many questions in vital statistics we have to calculate effects arising from causes whose nature and intensity,—nay, perhaps their very existence,—are all wholly unknown. When a medicine is brought forward laying claim to the power of producing a certain action in the living system, or of curing a given disease specifically, no matter by what process, nothing can be plainer than the *method* of ascertaining whether the allegation be well founded. Simply take care that genuine samples and similar doses of the medicine are employed, that all the individuals to whom it is exhibited are, as nearly as possible, circumstanced alike, and that the number of patients is sufficient, and then the conclusion that is educed by the accurate use of numbers may be considered to be as certain as any that can be obtained in a science that is not purely mathematical. Medicine cannot attain the exactness of astronomy or optics; but there seems to be no reason why it should not acquire equal certainty with chemistry, and other branches of experimental science.

If the efficacy of every new remedy had been thus tested as it arose, how often would the profession have been spared the humiliation of reposing unbounded confidence in agents which were really either inert or pernicious. Iceland liverwort would not then simply have settled down into a very respectable article of *diet*, after having promised to scare consumption from the face of the earth, nor would mercury have poisoned those myriads of persons who have fallen victims to an indiscriminating belief in its specific powers.

It is impossible, day after day, to observe the mass of isolated facts that are thrown before the profession relating to medicine, without lamenting the neglect to which we have drawn attention—both on this and many other occasions—and continuing to press the necessity of a remedy for the evil, until the proper remedy is adopted.

In the letter of Mr. Macilwain, which will be found at page 189 of this week's Lancet, that gentleman announces that the Council of the College of Surgeons recently preferred to him a junior member of the College, as a colleague in the governing body of the College. He charges the Council in that proceeding with having been guilty of a gross breach of trust under the Charter, and being influenced by mercenary impulses in their decisions, generally. He refers, in illustration and support of his assertions, to the principles on which the self-elections in the Council are alleged to be conducted; describes his own claims, as an officer of public medical charities, a teacher, an author, and a reformer in medical science,—to the appointment which was bestowed on a

younger member of the College, challenges his examination into his worthiness for the office, objects to the election of mere routine "lecturers" into the Council, and denounces as undignified, pernicious, and disgusting, to the last degree, the whole system of the Council as rulers in the profession.

How many other members of the College were "passed over" in order to arrive at the fit tool, Mr. MACILWAIN does not name. It would be as well to enumerate them, publicly,—NAMES and ADDRESSES.

By the last quarterly return of the Registrar General, showing the amount of mortality during the quarter ending September 30th in 114 of the town and country districts of England and Wales, it appears, —that in the metropolis the deaths amounted to 2.4 per cent. of the population (as per census of 1841); in the district of Brighton to 2.3; the Isle of Wight, 1.8; Portsea island (Portsmouth, &c.), 2.2; Windsor and St. Alban's districts, 1.9; Northampton and York ditto, 2.4 each; Norwich, 2.5; Exeter and Birmingham, 2.7 each; Plymouth, 2.4; Bath, Dudley, Walsall, Coventry, and Derby, 2.6 each; Ashton, Leeds, Macclesfield, Bury, and Sheffield, 2.7 each; Wolverhampton, Nottingham, Bolton, and Salford, 2.8 each; Manchester 3.2; and Liverpool, 3.5; the average mortality of England being about 2.2 per cent. Thus it would seem, as a *general* rule, and taking the great towns only, that as we proceed northward and westward, the rate of mortality increases, until it reaches its acmé in Liverpool. North and east of that point, again, the rate diminishes very considerably; for at Halifax it was only 2.1 per cent. in the last quarter; at Huddersfield, 2.0; at Newcastle-on-Tyne, 2.7; at Tynemouth, 2.3; and at Cockermouth no more than 1.8 per cent. on the population. And in many of the foregoing districts in which the rate of mortality was lowest, it is said to have been actually higher than the summer average; this was the case in Portsea Island, Devizes, Dorchester, Wigan, Huddersfield, Halifax, and Bradford, as well as Chorley, Derby, and Manchester. On the contrary, in the districts of Brighton, Colchester, Norwich, Plymouth, Bath, Bristol, Worcester, Dudley, Walsall, Birmingham, Leicester, Bury, Ashton, Leeds, Sunderland, Newcastle, &c., the rate of mortality was below the summer average. In all the 114 districts, 36,655 deaths were registered, being 428 less than the average of five preceding summer quarters, as calculated on the population at the time of the last census; but if considered with reference to the probable population at present, it gives a decrease in the mortality of about 2400 deaths below the average of the five past summers.

The districts of Windsor, St. Alban's, and Wycombe, have been peculiarly healthy; and at Northampton the registrar reports that the deaths have been fewer by one quarter than in any September quarter since the operation of the Act. At Cambridge, deaths from measles have been numerous, and at Bedford and Ipswich the mortality was above the average. Scarlatina has prevailed in the districts of Hereford, Devizes, and Dorchester, and of a very malignant form in several parts of Cornwall. Typhus, diarrhœa, and measles, have been very prevalent in the Lincoln district, but the first not very fatal. Nottingham has been comparatively very healthy, as was Sutton, near Macclesfield, till the middle of September, when diarrhœa became fatally prevalent. The mortality in a part of Liverpool is rather above the average, hooping-cough, measles, and inflammations of the lungs and bowels having been the prevailing diseases; in another part the return is below the average. Diarrhœa and typhus have prevailed at Bolton, and many severe cases of cholera at Chorlton; diarrhœa has been particularly fatal at Manchester; it was also prevalent, but much less fatal, at Ashton and Oldham. At Sheffield a remarkable preponderance of male over female deaths was noticed. At Holmfirth, near Huddersfield, and in several parts of the Bradford district, hooping-cough has been the prevailing disease. Diarrhœa, in children, and scarlatina, have been the principal maladies at Leeds. York and Tynemouth were particularly healthy; in the latter place no epidemic appeared throughout the summer. Typhus, though not generally fatal, has prevailed in Kendal, and also at Holywell (Wales). In most of the Welsh districts named, scarlatina has been the prevailing epidemic; but at Malpas, near Wrexham, hooping-cough. (The above is merely an abstract of the remarks of local registrars appended to the quarterly report).

MORTALITY AND METEOROLOGY OF THE METROPOLIS. — During the quarter ending September 30th, 2619 persons died of epidemic or contagious diseases (being 187 above the average of the five past summers); 1844 from diseases of the brain and nervous

system (109 below the same average); 2706 from diseases of the respiratory organs (136 below ditto); 281 from diseases of the heart and large vessels (72 above ditto); 1132 from diseases of the stomach and digestive organs (119 above ditto); 66 from those of the urinary organs, 114 in childbirth and from uterine, &c., diseases, 77 from diseases of joints, and 17 from those of the cellular tissue, &c. (the average numbers being respectively 58, 102, 68, and 13). 1200 persons died from dropsy, cancer, debility, and diseases of unascertained seat, and 667 from natural decay (averages 1272 and 694); 335 from violence, privation, or intemperance, 33 from causes not stated (averages 298 and 66); and the total deaths for the quarter were 11,091, or 71 above the quarterly average for the five past summers. "In the metropolis, diarrhœa and dysentery have prevailed to rather an unusual extent, the deaths from the two diseases having been 572, which is 262 above the average. Smallpox proved fatal to 75 persons, and scarlatina to 543, which is 99 more than the quarterly average. Pulmonary diseases were less fatal than usual; fewer deaths were ascribed to convulsions; more to tabes mesenterica and to cancer.

"The barometer was higher than the summer average (its mean 29.961 inches); the temperature never rose so high as in the summers of 1841 and 1842; but the mean temperature, 64°.6, was higher than in 1842, and 2°.5 higher than the average of ten summers ending in 1840. The fall of rain was 5.662 inches, which is near the average, but it fell on only 23 days. The season has been fine and warm, the south and west winds prevailing."—*Reg. Gen. Report.*

REMONSTRANCE

ADDRESSED TO

THE COUNCIL

OF

THE COLLEGE OF SURGEONS IN LINCOLN'S-INN-FIELDS,

IN OCTOBER, 1843,

BY MR. MACILWAIN,

A MEMBER OF THE COLLEGE,

"PASSED OVER" BY THE COUNCIL

AT AN ELECTION INTO THAT BODY

IN JULY LAST.

To the President and Council of the Royal College of Surgeons of England.

GENTLEMEN,—Having, with some difficulty, at length ascertained that on the vacancy in the Council, consequent on the death of Sir Astley Cooper, you passed over, amongst others, my name, and elected a junior member, it appears to me that I should neither act justly to the science that I profess, to the profession to which I belong, nor to myself, if I did not do all that an individual can do to resist such an aggression, viz., to record my protest against it, and the reasons on which it is founded.

In effecting this I would avoid any expression that is unfitting in an address to a public body. In venturing to arraign your proceedings I would discard everything which does not admit of proof. But if I think that you have violated the confidence which is reposed in you by Government,—that your acts rather represent those of a trading, than a scientific, community, and if one of these implies a personal neglect, nothing but a cowardice which I repudiate would excuse me from protesting against your injustice, or for repressing the language of complaint and expostulation.

Gentlemen, it is reported, and with too much appearance of truth, that the seats in your Council are reserved, as far as possible, for the surgeons of the London hospitals. If this be so, it is, in my view, an enormity of the most detestable character. Impossible, as it is, in an address of this kind, to unfold all its repulsive attributes, I will content myself with objecting,—first, that the positions in question are obtained, notoriously, by means which are altogether irrespective of professional or scientific merit; secondly, because it would be easy to show that, within the last twenty years, to no class of persons has science been less indebted than to the surgeons of the London hospitals, and this, even, were we to discard any allowance for the opportunities that they monopolise; thirdly, because in so restricting your selections for the Council you pass over men who, when they *have* wrought industriously, must have done so under comparatively disadvantageous circumstances, seldom enjoying equal facilities in collecting materials, scarcely ever equal opportunities of enunciating their results, and, therefore, men whom you ought especially to have encouraged, because it is not in the nature of things to suppose that a man will work under peculiar disadvantages, unless he be animated by more than an usual love of science; and, lastly, because, in such restrictions, you abuse the confidence which your Charter reposes in you, which by no latitude of construction can be said to empower you to maintain so odious a monopoly.

Unhappily, Gentlemen, and repulsive as it may be to me, I must now speak of myself; for as I would disclaim founding any pretensions on mere seniority, I must advance some of a more substantial character.

First, then, I submit that my early life was marked by labours at the hospital for an unusual number of years, and if not characterised by singular or unexampled, at least by very uncommon, industry; and this after my public and private occupations had

so increased that I might well have been excused from the additional labour of hospital attendance and inspection. This some of your own body can testify, as they can, also, Mr. Abernethy's notice of it.

I then held, for many years, institutions which I am prepared to show exacted greater responsibility than any of the hospitals, *as they are at present constituted*, whilst they required infinitely more labour, much greater nerve, and more varied talent and resources.

Before I had held any institution a year, I gave regular courses of clinical lectures, of which it is material to observe that they were delivered without remuneration, and also were the *first that ever were delivered in London*, a fact, which is alone, as it appears to me, worthy of more kind consideration by the College.

It was also at about this time that, in endeavouring to consolidate ophthalmic with general surgery, I succeeded, for the first time, I believe, in rendering operations for the eye a leading part of the operations of a general dispensary, performing, in the first ten months, twenty operations for cataract only.

From time to time I published such results of my inquiries as a reference to the wants of science, determined *by others*, suggested, and I contend that they are all positive additions, sometimes to the art, but more commonly to the science, of the profession.

The work on stricture adapted the various modes of treatment to their respective cases. The positions therein assumed have not, in any one instance, been shaken, whilst, so far as I know, the plan of the work was entirely new.

Finding that in the Truss Society, instituted for the relief of hernia, I became acquainted with a vast variety of other complaints, and that as a consequence of their having been mistaken by my professional brethren who sent them (often gentlemen of great eminence), I published the diagnoses of hernial and other tumours in the inguinal regions of the body, and I believe I say nothing but the truth when I assert that experience, and every test to which they have been subjected, has confirmed the truthfulness of such diagnoses.

At about this time, to a second edition of the book on stricture, I added remarks on the rectum and œsophagus, and a paper suggesting the earlier performance of tracheotomy in chronic laryngitis.

I also (as helping the downfall of local surgery) published on porrigo, in order to show that that class of cutaneous diseases which, while they were made to depend for their relief on local remedies, were most unmanageable, became, when properly treated, emphatic examples of the triumph of constitutional surgery.

I will now only allude to two of my other works, namely, those "On the Unity of the Body" and "Medicine and Surgery One Inductive Science." In those works, besides pleading for a more inductive method of pathological investigation, and showing how it might be obtained, I have demonstrated the effects of a more sedulous study and record of phenomena, first in a practical application (in the improved constitutional treatment of diseases) of those phenomena of the nervous system a very small portion only of which is included in what is called *reflex action*; and, secondly, I have enunciated a generalisation which, whether you admit it to be a law, as stated, or an axiom in approximation thereto, I contend is the most important generalisation at present reached,—that in assigning places and uses to facts hitherto not at all understood, in eliciting the causes of our failure and success respectively, it does more to overthrow mischievous *dogmata* and to divest our medical surgery of its conjectural character, and mould it into the form of real science, than anything which had preceded it. All this, I submit, is proved, even on the imperfect induction of the first volume (the only one published), but I pledge myself to manifest that it will stand the largest induction which pathology affords; and, if the opportunity be allowed me, Gentlemen (*which it ought to have been ere this*), in the theatre of our College, I could mention many others; but those claims which I have stated, and which the *nature* of my address *obliges* me to bring forward, are sufficient for my present purpose. I am not insensible how small these and all others are, in an absolute sense, especially had I enjoyed either health or fitting *arenæ*; but, still, in their *relative aspect* (and in this view I shrink not from comparison with any of your own body), I must feel that if they had not entitled me to distinction, they should, at least, have shielded me from neglect. I have heard, indeed, that my endeavours to introduce a more inductive method into our pathological investigations is the very reason why I am unpopular amongst you; if so, I must be content to pay the price of your displeasure.

But, Gentlemen, if I have alleged that our pathological investigations present a series of violations of the principles of inductive philosophy,—if I have asserted that our collections of morbid anatomy would be more fittingly characterised as "cabinets of curiosities" than as representing the materials for philosophical generalisation,—if I have said that your records of cases omit many facts having obvious bearing on disease, and present inductions of phenomena so imperfect that they would scarcely be accepted in any other science,—if I have represented hospitals that can find funds for the building of schools, dissecting-rooms, even domiciles for students, and all the machinery of the trade of teaching, but which have neither found

sufficient sympathy for the afflicted, nor perceived the important bearings of such a provision as to found one single "House of Recovery," although it involves an agency which is recommended in *private* practice every day,—if I have represented these institutions as so far badly conducted; and, lastly,—if I have said that it would be little short of madness to expect medical science to advance whilst those means are neglected to which all other sciences have owed their progress,—who is there among you, Gentlemen, that will dare to deny these allegations? You have plenty of opportunity, if you will, and although that very public, to the enlightenment of whom no one seems more opposed than our collegiate bodies, is beginning to retaliate by believing anything and everything that is said against us, yet the age is not so dark but that an unfounded allegation may be easily disproved.

In conclusion, Gentlemen, I would respectfully ask you, Is it possible that a retrospect of the last twenty years renders you so satisfied with the facts enunciated in your theatre as to feel that you have done wisely or profitably in restricting your choice of lecturers to those who had all their lives been addressing pupils? Or did it never occur to you that there might be men who could exemplify that the real study of the profession might be more auspiciously pursued when disincumbered of the trade of teaching? Look forth, I pray you, from the narrow precincts of the College, abroad into the profession; try some of the yet untried, and, therefore, at least not yet "found wanting;" and although you should begin with so humble an individual as he who now addresses you, *I pledge myself to show you simple exemplications of improved modes of study, productive of fruits which are solid increments to our knowledge*, which, whilst they are in no way more valuable than in their application to practice, have been deduced from sources, and by modes, which not only those who study the physiological, but our brethren in other departments of science, shall allow to have been deduced through unquestionable channels and by means of strictly logical ratiocination.

I am not so inexperienced as to expect your sympathy, nor blind to the uselessness of expostulation unsupported by power. Yet have I this consolation, in common with all who have not paid any of you an apprentice fee (for you have rejected a surgeon of a hospital* who had observed this formality), that each and all of us may consider that in the sacrifice of our just claims we have lent assistance to the overthrow of a system which, I believe, every unbiassed man regards as pernicious—as abhorrent to the interests of science—which lights up into an unextinguishable ignition, those heartburnings and jealousies that are so disgraceful to a profession that, when properly pursued, is full of the most exalting tendencies,—a system which tends to repress the struggles of talent and the hopes of industry, and to deprive that single-mindedness and love of science (in no profession so valuable) of its legitimate reward,—a system which does no one thing to point out or smooth the path to real professional eminence, but rests on an early perception of mercantile dependancies that ought to be unknown, and as early a subscription, both of mind and purse, to a conventionalism, as great an abuse of power as it is of the dignity of our profession.

I have the honour to be, Gentlemen,
Your obedient servant,
GEORGE MACILWAIN,
Author of "Medicine and Surgery One Inductive Science," and other works; twenty years Surgeon to the Finsbury Dispensary, &c. &c. &c.

* Mr. Lloyd, of St. Bartholomew's.

MEDICAL SOCIETY OF LONDON.

Monday, Nov. 6, 1843.

Mr. PILCHER, President.

CASE OF LARGE POLYPUS OF THE WOMB DETECTED DURING PARTURITION.

MR. CRISP exhibited a morbid specimen, of which he related the following particulars:—The patient was a lady, thirty-six years of age, and pregnant Aug. 14, 1843, with her sixth child. She had been delivered of her fifth child, by an easy and natural labour, nineteen months previously; she had since miscarried, and had previously done so in 1840, and twice before that period. During the last month or six weeks of the present pregnancy she had been subject to frequent small discharges of blood from the uterus, but had not been prevented from attending to her usual domestic duties. The liquor amnii was discharged a few minutes before Mr. Crisp's arrival, with a small quantity of blood. On examination the os uteri was found dilated to the size of half a crown-piece. The presentation could not be detected, but was supposed to be placental. The next day she was better, but fancied she should not recover. On the 16th Mr. Crisp was summoned in haste, and found the child had been expelled three or four minutes before he reached the house. She had been in labour only a few hours, the pains being severe only the last half hour. On placing the hand over the uterus he fancied he felt a second child, but soon altered his opinion, as the uterus appeared contracted. The placenta was retained, and at the end of three-quarters of an hour he introduced his hand, and removed it from its attachment near the fundus. He felt, on withdrawing his hand, a soft substance like a child enclosed in its membranes, and endeavoured to peel away from the side of the

uterus what appeared to be the placenta, but failing in this he attempted to perforate it; being foiled in this also, he desisted from the attempt. Mr. Bristow's opinion was now requested; he at first thought there was a second child in the uterus, but subsequently considered that it was a tumour. On further examination this was felt to be the case, and the tumour was found to be of the polypoid kind, and appeared to be attached by cellular tissue to the left and posterior part of the womb, extending to its fundus. Dr. Chowne was also consulted. He was of opinion with the other gentlemen as to the nature of the case. The after-history of the case developed a state of collapse, with powerful expulsive efforts of the uterus to get rid of the tumour. The patient was supported by slight stimulants and beef-tea, and camphor and opium pills. The catheter was introduced with some difficulty, and the water drawn off on several occasions,—the difficulty was owing to the presence of a portion of the tumour in the vagina. At last the catheter could scarcely be passed at all. It appeared to be in the bladder, but only a few drops of urine escaped. The bearing-down pains continued getting stronger; the patient gradually lost strength, her countenance becoming more collapsed and her pulse more frequent. She died on the morning of the 20th, four days after the delivery of the child. The abdomen from the commencement had been free from tenderness. The body was examined twenty-four hours after death. On examining externally, a large projecting body was seen at the lower part of the abdomen; on pressing this indistinct fluctuation could be discovered, which had not been evident before death. A part of the tumour, as large as an orange, dark-coloured and fetid, was protruding from the vagina. On opening the abdomen the bladder was seen much distended, the tumour pressing on the lower part, so as to elongate the urethra, and press the lower part of the bladder against the pubis. The projecting part of the tumour was now pressed back into the uterus. On removing the water from the bladder the uterus was seen much enlarged by the tumour. The fundus was empty, and as high up as the umbilicus. The uterus, vagina, and bladder were now removed. On cutting into the uterus the tumour, a "fibrous polypus," soft, elastic, and composed of cellular bands, was seen attached to the right side above the neck. The circumference of the attachment measured ten inches. The tumour was fourteen inches long, fourteen and a half in circumference at its largest part (the middle), and it weighed with the uterus seven pounds. The inner lining of the uterus presented a healthy appearance.

In his remarks on this case Mr. Crisp referred to some cases of an analogous kind, particularly to one described in "Loader's Journal," in which the woman died before delivery, from a similar tumour obstructing the passage of the child. He referred also to a case detailed by Dr. Gooch, and referred to by Mr. Travers, as an example of death occurring from pain alone. In this case the child was born alive, version having been resorted to. The weight of the tumour was three pounds fifteen ounces. The points of interest in his own case Mr. Crisp considered to be,—First, the time of formation of the tumour. Whenever it commenced it was highly probable that its chief increase was during the last pregnancy. No enlargement was observable in the abdomen prior to this period; it was likely, however, it existed of small size previously to the last miscarriage, and had had some influence in causing it. The second point of interest was the little inconvenience experienced during pregnancy in consequence of the presence of the tumour. This appeared, however, by reference to cases, as the general rule, although in some instances of smaller polypi fatal floodings had taken place during pregnancy. The situation and yielding nature of the tumour, as also its adhesion to the side of the uterus, would explain the little obstacle it offered to the parturient process. With respect to the error in supposing the tumour to be a second child, he felt convinced that most practitioners would have made the same mistake, and he referred to cases by Merriman and Smellie in support of this opinion. From the first the prognosis was unfavourable. The difficulty of passing the catheter was a point of interest.

Dr. Chowne remarked, that the inference of the gentlemen who saw the patient after the birth of the first child, from the large size and general character of the abdomen, that there was a second child, could not be a matter of surprise. The appearances warranted that opinion, and in similar cases, even when the internal examination was made, the first impression conveyed by the touch was usually that there was a second child. Upon making the internal examination there were three reasons for considering that the tumour was attached to the fundus; and, firstly, that it was considered, and indeed stated by authors, that tumours of the uterus, if they could be touched while small, were generally attached either to the os uteri or were not far within the cervix. That if a large tumour, when admitting of being touched, was *still* in the uterus, it was most probably attached to the fundus; that it would in that case distend the uterus and acquire considerable size prior to its protruding into the vagina. Secondly, when a gentle attempt was made to examine more towards the fundus of the uterus, progress was arrested by very perceptible adhesions over a considerable surface; and at one part

the firm root of the tumour was felt, although the full extent of it was not ascertained. Thirdly, the fact of the tumour remaining quite within the uterus long after the child had been expelled. The enormous size it had acquired, probably, we might presume, chiefly during pregnancy, would be the more astonishing were it not for the well-known vascularity of the uterus at the time of pregnancy; and, moreover, that in some instances tumours forming in the walls of the uterus carried a layer of the uterine muscular substance with them. A case was recorded by Bach in which the placenta adhered to the tumour, and thence derived the needful supplies. An obvious peculiarity appeared to be that a tumour of this immense size should have its roots low down, almost at the lowest part of the uterus, and its other extremity in the fundus and body of the uterus, and yet be retained wholly within the uterus, not only until the child was born, but for many hours after. Tumours sometimes, however, were attached to the surface of the uterus, over a very great extent, by cellular substance, which attachments were afterwards broken away by uterine contractions, allowing the tumour to descend; and this appeared to have been the case in Mr. Crisp's patient. With regard to treatment, Dr. Chowne could not have recommended any interference with the tumour while it was within the uterus in the condition described, and the patient being a woman just confined; and when the tumour had been detached from that situation by uterine efforts, it had been done at the expense of the patient's vital powers, and was followed by exhaustion, which proclaimed too plainly an inevitably fatal result.

Mr. CRISP, in answer to a question, said, that the tumour was not very vascular; the venous was much greater in proportion than the arterial circulation. He thought the tumour might have been removed by a ligature, but would not the woman have sunk rapidly had the growth been suddenly taken away. The tumour consisted of cellular bands, with a gelatinous kind of fluid between them. Looking at its diameter, its elasticity, the readiness with which it was moulded to any shape, that its base was small, its middle large, and its apex pointed, he had no doubt the tumour was a true polypus. When he first believed that the tumour was a second child enclosed in its membranes, and that he felt the placenta and attempted to detach it, his object was to reach the second child as quickly as possible, and bring it away.

Mr. HEADLAND questioned the propriety of the attempt to detach the placenta of the second child, as practised by Mr. Crisp. With respect to the tumour he thought it involved too much of the structure of the uterus to be a common polypus, it appeared to be more like a morbid development of the structure of the uterus itself. He thought a ligature could be scarcely applied in this case, judging from the difficulty of introducing the catheter. Nothing would have saved this woman, but the bold operation of entire removal of the tumour by the knife would have been as justifiable as many operations which had been performed in reference to the uterus.

Mr. DENDY agreed in the last opinion. He did not think the tumour a true polypus.

Dr. WALLER said that the tumour was of a more lobulated character than a polypus, and had no distinct pedicle. He mentioned a case of a somewhat similar kind, occurring in the practice of Dr. F. Ramsbotham, and in which, in consequence of hæmorrhage, that practitioner detached a fibrous tumour, having a pedicle, with the hand, and the woman did well. In Mr. Crisp's case he could easily believe that any practitioner might mistake the tumour for a second placenta, its structure being so similar.

Some little "skirmishing" took place between two or three of the members respecting the possibility of applying a ligature in this case, and also on the question of removal by the knife. The "firing" was rather sharp, but not very effective in producing materials for a report.

WESTMINSTER MEDICAL SOCIETY.

Saturday, November 4, 1843.

Dr. REID, President.

DISEASES OF THE URETHRA AND THEIR CONSEQUENCES.

MR. STORKS related the following case:—J. G., ætat. 35, was under the care of Mr. Whidborne, of Queen-square, for an attack of fever and cold, at the commencement of August in the present year; when convalescent he directed the attention of Mr. W. to a small encysted tumour near the anus; it was regarding this that Mr. Storks' opinion was requested on the 14th of that month. On examination a small encysted tumour was found close to the margin of the anus, and, from some remarks made by the patient, Mr. S. was induced to suspect some disease of the urinary organs. On inquiry he elicited the following particulars:—Five years ago the man suffered complete retention of urine, after an attack of gonorrhœa (of which he had had several), on which occasion Mr. Samuel Cooper passed the catheter, and subsequently several bougies, with complete relief to all his symptoms. Since that period, however, he had neglected to pay any attention to his urethra, with the exception of a bougie being passed by a chemist on two or three occasions some time ago. He complained of great irritability of the bladder, constant gleet, and a diminution in the stream of

urine; several hard, knotty points could be felt through the integuments in that portion of the urethra anterior to the scrotum. On introducing a catheter several tight strictures close to the meatus were found, on passing the instrument through which it was again arrested by a stricture in the membranous portion of the canal. Mr. Storks desisted on that occasion from any further attempts to pass the catheter into the bladder, and declined interfering with the tumour till the urethra should be in a more healthy condition. On the 16th, No. 3 was readily introduced into the bladder. Mr. S. again visited the patient on the 24th, at the request of Mr. Whidborne, and found him with the pulse 120; skin hot and dry; tongue coated with a brown fur; a chamber utensil contained a quart of urine which had been passed in a tolerably sized stream since the morning. On examining the perineum a small undefined tumour was found just behind the scrotum, which was much swollen and œdematous. On inquiry it appeared that some fever and general indisposition had shown itself on the 18th, but it was not till the 21st that any tumour was detected in the perineum, and the scrotum remained of its natural size up to the previous day (the 23rd). Mr. Storks looking upon the case as one of partial infiltration of urine, with the concurrence of Mr. Partridge, of King's College, made an incision in the mesial line of the perineum, from just behind the scrotum to the verge of the anus; this gave exit to a quantity of urine mixed with some fetid pus; and on the operator introducing his finger into the wound he discovered a large cavity running backwards to the left buttock, to lay open which it was necessary to prolong the incision to one side of the perineum, making it terminate, as in the operation of lithotomy, at a point midway between the anus and left tuberosity of the ischium. The case ultimately did very well after the separation of some sloughs of cellular tissue. Mr. Storks remarked that he considered the above case as one possessed of much interest, and deserving the attention of the society. Infiltration usually took place after complete obstruction to the passage of the urine, when, from the long-continued pressure upon that portion of the urethra immediately behind the stricture, ulceration was induced, and the contents of the bladder were effused into the cellular membrane. The instance just referred to, was, however, of a very different character, there being no total, and, indeed, a very partial obstruction to the passage of the urine. He considered it one of a class of cases well delineated by Mr. Abernethy in his "Surgical Observations" as examples of the urethra having given way, irritation producing ulceration. Mr. S. did not recollect the particulars of the four or five cases referred to by Mr. A., but in all of them he well remembered there was little or no obstruction to the passage of the urine. In the first case detailed by that authority the patient had been attended by a physician for typhoid fever; no allusion had been made to any morbid condition in these parts, and it was not till an attempt was made to introduce a clyster-pipe that any tumour was detected in the perineum. Mr. Abernethy had made some most valuable remarks to assist the surgeon in his diagnosis as to what portion of the urethra had given way, as well as some general observations on abscesses of the perineum, well worthy of perusal.

Mr. Chance had met with a somewhat similar case to that of Mr. Storks, occurring in a gentleman who, for two years, had been subjected to some obstruction in the urethra, but on no occasion ever amounting to positive retention. On one occasion, however, retention did occur, and then a catheter, No. 4, was passed easily into the bladder after encountering two strictures, one anterior and one posterior to the ligament. A tumour formed in the perineum, which the surgeon in attendance punctured, and found to contain urine. The tumour was subsequently laid open, and the patient is now doing well. The opening still exists, but it is much smaller than it was originally.

Mr. Rutherford Alcock remarked that cases similar to that related by Mr. Storks were often exceedingly embarrassing to the surgeon; in all probability the ulceration in this case was the result of simple irritation in the urethra. He had seen cases in which irritation in the urethra had produced ulceration, though acting in a different manner from that observed in Mr. Storks' case. An elderly gentleman had been subject to stricture for a great number of years, but had delayed resorting to instrumental treatment for its relief, in consequence of every attempt to pass a catheter or bougie being immediately followed by an attack of ague. This was dependent on the circumstance of his having served at Walcheren and suffered from the fever which was so prevalent among the troops sent on that expedition. On one occasion, however, the retention of urine was so nearly complete that only a few drops passed; there was great irritation of the neck of the bladder; the bladder itself was diseased, and his life was in jeopardy. Under these circumstances a small bougie was passed into the bladder with the hope of gradually increasing the size of the instrument, so that the urethra might be sufficiently dilated, but on every successive effort to do so such violent spasm ensued, followed after the second or third attempt by a violent attack of fever, that it was deemed advisable to desist. A fatal result was anticipated. The patient was sixty years of age. Sir B. Brodie was now consulted. The bladder-symptoms continuing, a small catheter was introduced, and, to the surprise of Sir Benjamin and himself, passed into the

bladder. It was now found, on inquiry, that on that morning the patient had suddenly passed a large quantity of pus. The obvious inference, therefore, was that suppuration had taken place in the urethra and had cleared away the strictured part in its membranous portion. The stricture, being less organised than the surrounding structure, more readily yielded to the suppurating process. The catheter afterwards passed quite freely. It was clear, from this and other cases of a somewhat similar kind that when there was considerable irritation in the urethra or about the neck of the bladder nature attempted to relieve herself, and succeeded. There was, however, a point, in reference to this class of cases, which it was advisable to bear in mind, and that was the dangerous and even fatal results which might follow retention of urine. He recollected the case of a young man in the Westminster Hospital many years since, who was admitted for stricture of the urethra and retention of urine for twenty-four hours. Some slight relief had been experienced by the dribbling away of a few drops of urine, and the bladder did not feel to be much distended; suddenly, however, and whilst in a warm bath, he was seized with convulsions and died. There was no rupture of the bladder, and no appearances whatever to indicate the cause of death. He appeared to have sunk from irritation of the nervous system. We should, therefore, in persons of a sanguine nervous temperament, not delay too long the emptying the bladder by artificial means.

Dr. REID inquired what was the largest quantity of water which any member had found to be retained in the male bladder? The largest quantity he had seen in females had been in cases of retroversion of the uterus.

Mr. CHANCE had, in a case of gonorrhœa in which it was necessary to use the catheter, removed a pint more water than would fill an ordinary chamber utensil.

Dr. GRIFFITH remarked that in all cases of unrelieved retention of urine the cerebral symptoms producing death arose from the same causes as those of idiopathic suppression of urine, viz., the presence of urea in the blood.

Mr. CHOWNE related a case of rupture of the bladder from external violences in which at the expiration of five or six hours, the catheter was introduced, and a pint and a half of water drawn off. The next day there was slight oozing when the catheter was employed. The man died, and the bladder was found ruptured at its superior part. It would appear that at first there was no escape of urine into the peritoneal cavity. The ruptured portion gaped, and no signs of adhesion were present.

Mr. ALCOCK had seen a case of gun-shot wound in which the ball passed through the under surface of the bladder. No urine escaped. The man died on the third day. He could understand that in a case like this the swelling and tumefaction might close up the wound and stop the egress of any fluid until inflammation and its consequent adhesion came on.

After a few words from Dr. Chowne, Mr. Chinnock, and other members, the society adjourned.

UNIVERSITY OF LONDON.

EXAMINATIONS for the degree of Bachelor of Medicine took place on Monday, Tuesday, and Wednesday last. The second examination in FORENSIC MEDICINE, Professor Daniell, Dr. Pereira, and Dr. Rigby, examiners, comprised the following questions:—

What are the characters by which you would distinguish spots of blood upon iron from common rust, and what is the principal ambiguity to be guarded against?—Under what circumstances may lead be acted upon by water, and how would you detect its presence?

What are the symptoms and chemical evidence of poisoning by nux vomica?—The death of an infant is suspected to have been caused by an overdose of laudanum administered medicinally. You are required to state the smallest dose of this liquid which in your opinion might produce death; to describe the symptoms which would arise; and, lastly, to state how you would proceed chemically to detect opium in the untaken portion of the medicine.

Three women are brought before you, in whose cases you are required to give your opinion as to the presence or absence of pregnancy, on the following data:—

1. A single woman denies that sexual intercourse has ever taken place. The last appearance of the menses was on the 1st of last September, and then very sparing; the areola is dark (she is a brunette, and is subject to irregular menstruation with much pain); there is no perceptible abdominal enlargement; the os uteri is easily reached; it is round and seems closed, and its lips tumid and soft; the cervix is also soft, and of the full size and length.

2. The mother of three children (married). She states that her husband, a sailor, left home on the 8th of last May; the last appearance of the menses was on the 22nd of May. There is a decided enlargement of the abdomen, the tumour reaching nearly up to the umbilicus. She denies that she has felt any movements like those of a child, nor have you satisfactorily detected any with your own hand. The os uteri is high up in the hollow of the sacrum; it is soft, round, and does not admit the tip of the finger; the cervix is shortened, the uterus above it feels enlarged, and a moveable tumour within it is indistinctly felt.

There is an areola, but she is of a dark complexion, and her youngest child is only a year old.

3. A married woman, has had six children, the youngest of them is six years old; she reckons from the last appearance of the catamenia, viz., March 8; it was scanty, and there has been a slight show twice since. The abdomen is as large as in a woman at about the eighth month of pregnancy; it is firm to the feel; the breasts present no satisfactory proofs of an areola (owing to the discolouration from former pregnancies); the os uteri is in the middle of the pelvis, forming a transverse fissure; the cervix is long and firm; a solid and somewhat moveable body, of about the size of a child's head, is felt in the upper part of the pelvis. She states that she quickened in July, since which she has felt the movements of the child, which, however, you have not as yet succeeded in feeling with your own hand.

If you consider any of these three women to be pregnant, state the period of pregnancy; if not pregnant, what is the nature of the case? State also the grounds on which you have formed your opinion in each.

The examination in PHYSIOLOGY, Professor T. Rymer Jones examiner, was thus arranged:—

1. In what classes of animals does a portal circulation exist? Describe the arrangement of the portal system in each class wherein it is found. 2. How is absorption effected in the invertebrata? State your reasons for the opinion that you give. 3. Describe ciliary motion. State the principal opinions of physiologists as to its cause, and the more important uses to which it is subservient in various races of animals. 4. Describe the respiratory apparatus of a perennibranchiate batrachian, and the general course of the circulation of its blood. 5. Contrast the auditory apparatus of a cuttle-fish, a fish, and a reptile. 6. What races of animals possess a distinct urinary apparatus? Describe the principal varieties in its composition and arrangement. 7. Describe the corpora Wolffiana.

[We shall take other opportunities of publishing the questions contained in the remaining examinations.]

ENORMOUS OPIATES.—In rupture of the uterus or of the intestine, opium, administered, appears to lose its narcotic and astringent properties. A man admitted into the Meath Hosptial, labouring under symptoms of peritonitis from ulcerative perforation of the intestine, took 105 grains of opium, exclusive of this drug in injections, without experiencing the slightest coma, headach, or delirium. — *Dr. Mitchell, in Dublin Med. Jour.*

DR. RANKING'S ESSAY ON SPERMATORRHŒA.

To the Editor of THE LANCET.

SIR,—As your correspondent "CHIRURGUS" (page 152), without establishing his right to judge me *ex cathedrâ*, has thought fit to publish an authoritative criticism on my paper on Spermatorrhœa (page 46), I might perhaps be justified in saying that when he appears in his own name it will be time enough for me to determine whether his strictures are worthy of my notice. As, however, his animadversions have appeared in THE LANCET, I think it due to you to say,—First, that I quoted Lallemand whenever I referred to him, and that it is of little consequence that this acute reasoner should have discovered my want of originality; and, moreover, that as the greater part of the profession may not be so erudite as "CHIRURGUS," and, therefore, possibly are not familiar with the work of Lallemand, I have not performed an useless task in bringing the subject before them. Secondly, that I had not read Mr. Curling's work on the Testes. If I had I should have referred to it, and with the commendation which it doubtless deserves. Thirdly, that when I learn that a competent observer, such as Bowman or Gulliver, has detected the presence of spermatozoa in the urine of spermatorrhœic patients, I will modify my assertion, but that I do not believe that the microscopist sees, in all cases, what he professes to see. Fourthly, the *small* criticism concerning Hunter and Sir A. Cooper I should imagine to be worthy of "CHIRURGUS." Fifthly, as to the application of lunar caustic, I freely grant to your correspondent all the triumph he can elicit from my anatomical *lapsus*. Sixthly, your correspondent's conclusion is based upon a wilful perversion of my meaning. I say I have "*caused*" it to be tried (the *italics* are his own), because as I am not one of those who abuse the title of "Doctor of Medicine," now, thanks to the new manufactories, to be had so pleasantly and cheaply, by promiscuous practice,—I prefer the old-fashioned mode of securing a surgeon's aid to do a surgeon's duty. Seventhly, in answer to the last piece of polite criticism, that I might have left to others who, like "CHIRURGUS" himself, no doubt, "possess a more perfect knowledge, the task of explaining to the profession," &c., I can only surmise that this overflowing of the gall of bitterness must arise from my having forestalled some observations of "CHIRURGUS" himself upon the same subject. If so it is to be regretted, assuredly, but I trust that we shall not on that account be deprived of information which, if it exhibit as much acumen as his efforts at criticism, must be highly desirable. It is, however, some consolation to think

that the profession in general does not see with the eyes of "CHIRURGUS."

I shall here take leave of him, in the hope that before he presumes to criticise he will first establish his right to talk upon the subject, for in the words of Coleridge, "until he can understand my ignorance, he must presume himself ignorant of my understanding." It may be as well to say, in conclusion, that I shall not notice any further communications which may appear under an assumed name. I am, Sir, your most obedient servant,

W. H. RANKING, M.D., Cantab.

P. S.—As a slight contrast to the spirit which pervades the production of "CHIRURGUS," I should feel greatly obliged by the insertion of the subjoined letter, from a perfect stranger, and I must apologise to him for thus making use of his communication:—

"To W. H. RANKING, M.D.

"Sir,—You will see that the editor of THE LANCET acknowledges the receipt of an essay on spermatorrhœa. I am the author of that essay, and, to my surprise, I found, on receiving the four last numbers of THE LANCET this morning, a comprehensive and excellent paper by yourself on the same subject, to which, had I seen it earlier, allow me to say I should have made honourable allusion, as it certainly contains much valuable matter. . . . . I consider it my duty to give you this explanation of my silence with regard to your essay, and remain, Sir, yours respectfully,

G. N. DANGERFIELD, M.D.*

Newport, Monmouthshire,
Nov. 2, 1843.

REGISTRATIONS AT THE HALL.

To the Editor.—Sir: Having experienced, with many others, excessive inconvenience in the last few days at the registration at Apothecaries' Hall, I beg to lay before you the new regulation, lately put in force, for publication in your valuable periodical. On arriving, a ticket is given you, and after waiting a considerable time, during which you are kept out in the court, without a chair to sit upon, the parties are ushered in, one by one. For example: I was there on the day before yesterday from one o'clock to half-past two, waiting to register; and another pupil, on the day before, who had arrived at twelve o'clock, was kept from that time till they left off registering, and then was told to come on the next day. This inconvenience may be easily remedied. Appoint an assistant or two to the almost superannuated secretary, the only person who at present officiates; then the registration might be effected without any loss of time to the students. I am, Sir, your obedient servant,

ONE OF THE MANY SUFFERERS.

ONE OF THE BY-LAWS OF THE COLLEGE.

To the Editor.—Sir: Being anxious to ascertain if a friend had passed the ordeal of the College in Lincoln's-Inn-fields on Friday evening last, I bent my way thither, and, as it was a very wet evening, I contemplated the shelter of the porch until my friend should come out. But I was disappointed. The gates were shut and locked, and admission was refused by the porter, "by order of the president." Yet that luxury which was denied to me and about seven or eight others (thus compelling us to walk up and down for an uncertain time in a pouring rain) is graciously granted to the various footmen and other servants who are attendant on the carriages. Really, Mr. Editor, this is abominable. What right has Mr. President to shut out any member, even from *the porch* of his own College? He could not overhear and detect there the wisdom of the examiners! If any other gentleman should be in the same situation I advise him to get over the wall, which was very easily done by

A MEMBER (NOT A FELLOW).

October 28, 1843.

SPURIOUS HYDROPHOBIA, AFTER RETROCESSION OF SCABIES.

A YOUNG unmarried woman, in Bohemia, twenty-six years of age, of nervous temperament, and occasionally subject to hysteric attacks at her catamenial periods, became infected with scabies. Warm baths and the utmost cleanliness proved of no avail as means of treatment,—Priessnitz's cold-water system was, to all appearance, more efficacious, for after about a month's affusion and imbibition of the pure element the itch disappeared. But there turned out to be only a change of evils; the patient became melancholy, sought solitude, suffered from palpitations of the heart, and convulsions of the muscles of the orbit, acquired a wild and insane appearance, and, finally, betrayed all the physical horror of water that characterises hydrophobia. Thirst, pain along all the course of the spine, little power of speech, and sleeplessness, were among the other symptoms. The disease could not be traced to any bite from a rabid animal, and the physician who reports the case gives us to understand that he had already found out that it was the consequence of a metastasis of scabies to the spinal cord and its mem-

* A note to the editor, from Dr. Dangerfield, mentioning his notice of the paper of Dr. Ranking, has been received by us for appendage to the essay of Dr. D.—ED. L.

branes! Though no friend to the hydropathists, for to their doings he attributed the aforesaid metastasis, he seems to have been stung by the homœopathic λυσσα and its doctrine of *psora*; for among his remedies he prescribed internally sulphur, with small doses of camphor, doubtless to bring the psora out again. True it is, that with this he adopted some more vigorous and less hypothetical practice, such as salt baths, cupping, and afterwards tartarised antimony ointment along the spine, and the internal use of morphia, sarsaparilla, and liq. antim. tart. Now, if the psora had shown itself again it should at least have been in its original shape—we ought to have had the real Simon Pure; instead of which the ointment rubbed down the back brought up a fine crop of herpetic pustules! The physician still attributed these to the same cause that had given rise to the original scabies, but now thought it high time to employ baths more eminently specific against the disease, and probably sulphur was the agent employed, and in a more legitimate way than previously. At all events, the patient is said to have got well in a few weeks; though, as the main symptoms which caused alarm bear evident marks of having been mere modifications of the Protean malady, hysteria, there is a likelihood that she would have recovered from these under a treatment much less severe than that put in force.—*Oest. Med. Wochens.*, Sept. 9.

BILOCULAR UTERUS.

A CASE of arrest of development is said to have occurred in Vienna, producing cleft vagina and bilocular uterus. The subject of it was a woman, thirty years of age, and who, being pregnant, applied to become an inmate at a lying-in charity. Externally she was well-shaped, and appeared robust; but on making examination the vagina, at the depth of about two inches, was found divided into a double passage by a dense fibrous septum stretching across it. The posterior chamber was penetrable by the finger for about an inch and a half higher, when it was found to end in a small blind sac. The anterior passage of the vagina was so long that the os uteri could not be reached by the finger; the fœtus accordingly lay very high in the pelvis. The birth was at first lingering, but in the progress of the labour the septum in the vagina spontaneously ruptured, with little loss of blood; the liquor amnii was immediately discharged, and in a short time afterwards a living child was safely expelled. The mother, however, died of peritonitis four days afterwards, and on opening the body the cavity of the uterus, as far as the os internum, was seen to be separated into two chambers by a vertical septum. The fœtus had lodged in the left of these divisions, but the right cavity had also been dilated and lined with decidua during the pregnancy.—*Oest. Med. Wochensch.*, Sept. 9.

PREGNANCY IN THE VAGINA.

ONE of the German journals reports a case of extra-uterine gestation in which the fœtus was developed in the vagina. A circumscribed enlargement was apparent between the navel and the pubis, and the bowels and bladder were evacuated with much difficulty. A practitioner appears to have been first called in at about the fourth month (die Geburt schon bis zur *vierten period* vorg. war), who found the fœtus in a cross position and dead. He immediately proceeded to delivery by the feet, and after much difficulty brought the shoulders through the vulva, and afterwards extracted the head with the forceps. The circumscribed tumour was yet unreduced, and on examination this was found to be due to the uterus itself. That organ was retroverted, its orifice being directed forwards to the abdominal integuments, and closely embracing the cord. The accoucheur contrived, however, to introduce some of his fingers within the os tincæ and remove the placenta, which is said to have been adherent to the neck, and, indeed, to all the rest of the internal surface of the uterus. The woman recovered satisfactorily. We know of only one other recorded case of this very rare kind of extra-uterine gestation; it is detailed in the "Journ. de Med.," &c., of Paris, 1779. The latter case terminated unfavourably to the mother.—*Oest. Wochensch. and Med. Zeitung*, 1843, No. 18.

NEWS OF THE WEEK.

COMPLAINT FROM EDINBURGH.—A correspondent, at Edinburgh, under the signature of "A Medical Student," in a letter dated November the 4th, has addressed to us the following statement:—"The subjoined circumstances have been, and still continue to be, looked upon by the students in this city as a great grievance. The courses of practical anatomy and anatomical demonstrations were taught in the schools here, up to the end of the summer of 1842, as separate classes, and the fee to each was two guineas. On the opening of the session in November, 1842, however, a meeting was held by the anatomical teachers of this city, on the subject, the result of which was that resolutions were agreed upon, and printed, and placarded in the various dissecting-rooms, to the effect that, as the anatomical teachers of Edinburgh considered that the fees for practical anatomy did not *sufficiently* remunerate them, it had been determined that the tickets of practical anatomy and demonstrations should be conjoined, and that, in future,

they should be issued as one ticket, instead of separately, as heretofore; the fee to be four guineas. This resolution was not made public until the last moment, when it was too late for any new lecturer to set up on the old principle. Many remonstrances were made against this proceeding by such students as wished to complete the curricula of those medical schools that did not require the course of demonstrations (the University and the College of Surgeons of this city, and many others); to which the only answer granted was that such were required by the College of Surgeons of London. Beyond this, no explanation was given, but the subject passed over with silent contempt, the lecturer at the same time pocketing his four guineas. The same course has been advertised to be continued in this session, and the hardship complained of is that students should be compelled, by a clique of money-seeking lecturers, to pay for a class which they did not require for any board before which they intended to appear, and which many were unable to attend from its being necessary to take out the ticket to a *required* class at the same hour. The matter can only be regarded by all fair-dealing persons as an infringement on, and a disgrace to, the liberal principles upon which the Edinburgh Medical School has hitherto been conducted. It is generally supposed that the scheme was suggested, and mainly carried out, by the members of an anatomical school for the first time instituted at that period.

Cheltenham.—A correspondent from this town writes as follows on a subject of a very great annoyance to the profession in most parts both of town and country:—"In this fashionable locality it is the fashion for the druggist to recommend either a certain physician or (so denominated) 'pure' surgeon to an invalid requiring professional aid, so that the druggist might have, in return for his trouble, a *good* prescription to make up, the medical man being lauded by the vendor of drugs as the cleverest person in the place. It is to be regretted that the surgeons, who are fully qualified to practise medicine and surgery, do not determine to eradicate this system, which is pursued also in other places to a vast extent. If something be not done, it will soon be useless for general practitioners to try to establish themselves anywhere. The only remedy that I can suggest is for them in each town to establish a company of their own, by shares, like that of the Apothecaries' Hall in London, where the public may procure good medicines, and the shareholders confine their prescriptions to being dispensed there by competent assistants, a list of the legally-qualified practitioners being kept constantly on view for the benefit of the public. This company would not exclude physicians from becoming shareholders, as it should be established on a liberal basis, instead of the principle of monopoly, which would be quite foreign to the legitimate object of the establishment."

Royal Medical and Chirurgical Society.—The first meeting of this society will be held on Tuesday evening next, Nov. 14th, at half-past eight o'clock, when the following papers will be read:—

"An Account of Two Cases of Rupture of the Ureter or Pelvis of the Kidney, followed by large Effusion of Urine into the Abdomen," by Mr. Stanley, president of the society.

"Two Cases of Ulcer of the Stomach producing Circumscribed Abscesses in the Peritoneum," by Dr. Edward J. Seymour.

Gift to the Norfolk Hospital.—The *Norwich Mercury* of last week mentions that at the quarterly meeting of the Norfolk and Norwich Hospital a donation of a "most valuable and important kind" was made to this institution by Mr. Dalrymple, in the form of a museum of pathological anatomy. "With sons in the profession," says the *Mercury*, "the presentation of a museum so valuable presents an instance of liberality and devotion to science and his profession which few would be found to possess. In addition to the labours of his own life are added the labours of one whom the kingdom was ever ready to honour; Mr. Martineau's collection, bequeathed by that gentleman to Mr. Dalrymple, is included in the gift." The more interesting objects of the collection were the products of Mr. D.'s practice at the hospital, accumulated during twenty-five years. "Now that my father's professional career," says Mr. John Dalrymple, in the letter conveying the gift, "has closed, and his personal services can no longer be of value, he desires to contribute what he yet may to its permanent utility and advantage. The Norfolk and Norwich Hospital is already possessed of the finest collection of urinary calculi probably anywhere in existence, and by it the reputation of the institution has been extended in no slight degree. It is presumed that additional value will be given to this splendid acquisition by the various specimens of kidney and bladder disease, and the morbid parts, the result of fatal cases of lithotomy." . . . "For my own part, and on behalf of my family, I beg to add, that it will ever be a source of pride and satisfaction to them to see it deposited in a place so appropriate as the scene of what I must be permitted to call his honourable labours." The grateful thanks of the governors of the hospital have been given to Mr. Dalrymple for his magnificent donation, and a committee was appointed "to consider the best mode of transmitting this resolution in a more permanent and complimentary form than was customary."

APOTHECARIES' HALL, LONDON

NAMES of gentlemen who received certificates of qualification to practise as apothecaries, Thursday, Nov. 2, 1843:—William Spackman, Lutterworth, Leicestershire; Dodd Perkins, Bridgewater, Somerset; Horace Crofts Hastings, East Dereham, Norfolk; Edward William Pilgrim; William Smith, Newport, Bucks.

BOOKS RECEIVED.

A Practical Manual of the General Chemical and Microscopical Characters of the Blood and Secretions of the Human Body, with a description of the best methods of separating and estimating their ingredients, and of the various concretions occasionally found in the body forming calculi. By J. W. Griffith, M.D., &c. London: R. and J. E. Taylor, 1843. Pp. 60, and plates.

The Oculist's Vade-mecum; a complete practical system of Ophthalmic Surgery, with numerous woodcuts and coloured engravings of the diseases and operations on the eye. By John Walker, Surgeon, Manchester. London: Longman and Co., 1843. Pp. 400.

Manual of Diseases of the Skin. From the French of MM. Cazenave and Schedel, with notes and additions by Thomas H. Burgess, M.D., Surgeon to the Blenheim-street Dispensary for Diseases of the Skin. London: H. Renshaw, 1842. Pp. 320.

CORRESPONDENTS.

A Constant Subscriber.—As the building named is not a public institution it does not belong to the class described in the clause of the Medical Witnesses' Act, which excepts the medical officers from receiving the fee. The surgeon, therefore (although bound to *attend*, as are *all* persons, on the *common* summons of the constable), should receive the "medical order," printed in the schedule of the Act, from the coroner before giving an opinion on the case, and on his production of that, at the close of the inquiry, he can legally claim payment for his evidence. If he be charged with neglect or culpable maltreatment of the person dead, the case may assume a different character. He should read the Act itself, which has twice been printed in this Journal. In reply to question the second we should say that if the functionary referred to held office when he attended the patient, "seventeen years ago," proof of the facts alleged would probably induce the Lord Chief Justice of the Court of Queen's Bench to decide in favour of his removal. But that proof it would be very difficult or impossible to establish.

M.—Our columns are at present so fully occupied with matter which is more acceptable to the profession that we shall not be able to devote space to the communications. The circumstance is the less material from the certainty that the author will publish them in one or more forms.

If the "case" supplied by *Medicus Rusticus* would succeed in preventing the silliness which it is designed to ridicule, it should have a place in our columns; but what is foolish and useless must be corrected in a more certain manner.

A duly-qualified practitioner contemplates joining in partnership with a gentleman who is a surgeon-dentist only, but neither a member of the College nor a licentiate of the Apothecaries' Company. Would the latter circumstance render the partnership illegal, or prevent the firm, on sending out their bills, from enforcing payment?—*Q. Q.* The partnership would not be illegal, but the "firm" could not recover at law in medical cases, because the last-mentioned practitioner, not being a legally-qualified apothecary, he could not successfully bring an action in that capacity. In his own name the licentiate could practise and recover for what he had himself done and supplied, whether a partner or not.

Mr. Giordans must communicate to us, unreservedly, in writing, the particulars of the treatment which he would employ for the cure of the "different maladies" that he enumerates, otherwise we certainly could not recommend him to any one or "half-dozen" afflicted persons (even if already "pronounced incurable") for experiment, notwithstanding the fact that Mr. G. would "find everything requisite" and willingly attempt the cures without remuneration. On describing, fully, the treatment to us (which, however, we hardly expect that he will), every proper attention shall at once be paid to his communication.

Cautus.—Of course his "opponents *could* object," but whether successfully, or with justice, we cannot undertake to say, since they have no legal power at their command wherewith to enforce their objection. Our correspondent should rather consider what objections the poor-law commissioners would raise to the double situation. We know that they could, and believe that they would, compel him to resign one of the two offices, namely, the professional, if appealed to on the subject.

M. B.—The gentleman has already been brought upon his knees, and it is not worth while, at present, to attempt his depression beyond that humble position.

We hope to have space next week for the essay of *Dr. Dangerfield* with several other communications, and some reports.

Mr. Harvey's request has been transferred to the publisher.

ERRATA.—In page 140, seventh line, first column, leave out "(as at fig. 2)." In page 147, thirty-ninth line, second column, the word ingulfs was misprinted "engulphs."